PROCEEDINGS
THE INSTITUTION OF CIVIL ENGINEERS

PART I
NOVEMBER 1952

ORDINARY MEETING

20 May, 1952

ALLAN STEPHEN QUARTERMAINE, C.B.E., M.C., B.Sc. (Eng.), President, in the Chair

It was resolved :—

That Messrs H. M. Bostandji, D. A. Brown, E. W. Cuthbert, R. W. A. Fane, Andrew Henderson, J. A. Johnston, E. C. Lightbody, C. W. Pike, and Harry Ridehalgh, be appointed to act as Scrutineers, in accordance with the Bye-Laws, of the Ballot for the election of the Council for the year 1952–1953.

The Council reported that they had recently transferred to the class of

Members

Bernard James Callinan, D.S.O., M.C., B.C.E. (*Melbourne*).
Harold Ernest Thomas Compton.
John Reginald Cone, M.Sc. (Eng.) (*Lond.*).
John Alfred Fisher.
Herbert Fagent Merrington, B.Sc. (Eng.) (*Lond.*).
George Martin Swan, B.Sc. (Eng.) (*Lond.*).

and had admitted as

Students

Don Sepala Herat Abeywickrema.
Brian Howard Adams.
John Derek Allen.
Sivapragasam, Allirajah.
Thiagarajah Shanmukh Anandam.
John Edward Barber.
Colin Brooks.
Michael Ernest Brown.
John Percy Copper Burch.
Thomas John Burke.
Donald John Chapman.
Douglas Church.
Terence Alexander Creaney
Keith Thomas Dee.

Robert Dobson.
Peter Brian Dugdale.
John Young Dutton.
Arthur Edward Eaton.
Stephanus Petrus Enslin.
Paul Pitt Fearnside.
Ronald Fellows.
Thanthulage Obesiri Piyadasa Fernando.
Edward Wasley Flaxman.
Martyn Gerald Garrett.
David Gear.
Paul Malcolm Genders.
Allick Grimwood.
Kenneth Peter Grubb.
William Arthur Hibbert.
Derek Ronald Edward Holland.
Mark Raymond Howells.
Peter Frederick George Hutchins.
Michael Courage Instance.
William Sommerville Jack.
William McLaren Jenkins.
Brian Edward Kendall.
Prem Prakash Khanna.
Philip Martin King.
Nathabandu Thilaksri Kottegoda.
Jerzy Slawomir Andrzej Kulczycki.
James Graeme Lang.
Alan Moorhouse Leggat.
Colin Walter Leitch.
Herbert Kenneth Michael Lugg.
William McKinlay.
Peter Clews Mornement.
Harry George New.
Oliver Bryan Nunnington.
John Odams.
Roger Henderson Oliver.
Koruwakankanamalage Don Piyadasa Perera.
Howard William George Polley.
Donald Sutherland Ross.
Gruffydd Owen Rowlands.
Thomas Anthony Ryan.
Peter Samuel.
Michael Boyce Scanes.
John Beaumont Shaw.
Jack David Shepherd.
Frederick Geoffrey Shutt.
Ronald Charles Sinclair.
Laurence John Soffe.
Frederick Campbell Stott.
Morris Hugh Taylor.
Trevor George Taylor.
John Frederick Rory Weston.
Paul Edward Wheeldon.
John Robert White.
Ivor Reginald Wong.

and had admitted as

Graduates

Michael Alexander Ernest Adams, B.Sc. (Eng.) (*Lond.*).
William John Richard Alexander, B.Sc. (*Witwatersrand*).
Gerald Alphonsus Allen, B.E., B.Sc. (*National*).
William Francis Attwell Allen, B.A. (*Cantab.*).
William Stanley Smith Anderson, B.Sc. (*Edin.*).
Dinos Nicolaou Aphamis.
Don Gunasena Athukorala, B.Sc. (Eng.) (*Lond.*).
William Bannatyne, B.Sc. (*Glas.*).
Nicholas Barker.
Kenneth Barlow, M.Eng. (*Liverpool*).
Geoffrey Ernest Barltrop.
Richard Glyndwr Barnes.
Thomas Kenneth Bateman, B.A., B.A.I. (*Dublin*).
Christopher John Beagley.
Leonard Montague Bell, B.A., B.A.I. (*Dublin*).
Colin Joseph Bennett.
Tom Graham Bentley.
John Graham Berry, B.A., B.A.I. (*Dublin*).
Richard Anthony Protheroe Berry, B.Sc. (Eng.) (*Lond.*).
Tyagaraja Bhaskaran, B.Sc. (*Madras*).
John Harold Bleasdale, B.Sc. (*Manchester*).
Dennis Oliver Body, B.Sc. (Eng.) (*Lond.*).
John Claude Botterill, B.Eng. (*Sheffield*).
Charles Richard Bower, B.Sc.Tech. (*Manchester*).
Cecil Betsworth Bowyer.
Malcolm Bowyer, B.Sc. (*Manchester*).
Alan Keith Braint, B.Sc. (Eng.) (*Lond.*).
John Crawford Bretton, M.A. (*Cantab.*).
John Cyril Briggs, B.Sc. (*Leeds*).
John Franklin Brining.
Robert Broadley.
Graham Dudley Brown, B.Sc. (Eng.) (*Lond.*).
Peter Brown, B.Sc. (*Leeds*), Stud. I.C.E.
Derek Walter Bryden-Smith, B.Sc. (Eng.) (*Lond.*).

Ronald Alexander Bryson, B.Sc. (*Belfast*).
James Buchanan, B.Sc. (Eng.) (*Lond.*).
Geoffrey Murray Buckley, B.Eng. (*Liverpool*).
Frederick Barnes Bunting, B.Sc. (*Belfast*).
Robert William Burrows, (B.Eng.) (*Liverpool*).
Gordon Bush, B.Sc.Tech. (*Manchester*).
Charles Joseph Bushell.
Morriss Charles Butcher, B.Sc. (*Durham*).
Alexander Alastair Wentworth Butler, B.Sc. (*Nottingham*).
Ernest Otto Butts.
Raymond Alfred Caine.
David Caldwell, B.E. (*New Zealand*).
Angus Murray Cameron.
James Carr, B.E. (*National*).
John Carruthers.
Donald Cartwright.
Peter Michael Chandler.
John Alexander Clark, B.Sc. (*Cape Town*).
Thomas Edward Clark, B.Sc. (*Wales*), Stud. I.C.E.
Michael David Clarke.
Donald James Cliffe, Stud. I.C.E.
Samuel Clinton.
George Francis Gilbert Clough, B.Sc. (Eng.) (*Lond.*).
John Thomas Codd.
Andreas Coetsee, B.Sc. (*Cape Town*).
John Edward Coleman, B.Sc. (Eng.) (*Lond.*).
Peter Comber.
Peter Campbell Cooke, B.A. (*Cantab.*).
Arthur George Hywell Coombs, B.Sc. (*Bristol*).
John Caird Corfield, B.Sc. (Eng.) (*Lond.*).
Charles Robert Cowlin, M.A.(*Cantab.*).
Lawrence Berkley Crossing, B.A.Sc. (*Toronto*).
Frank Wyndham Crowe.
George Crowther, B.Sc. (Eng.) (*Lond.*).
Herbert Finlay Cumming, B.Sc. (*Glas.*).
James Bruce Cunningham, B.Sc. Tech. (*Manchester*).
John William Dalziel.
Brian Francis Darbyshire, B.Sc. (Eng.) (*Lond.*).
John Gordon Pryce Davies, B.Sc. (*Belfast*).
Joseph Wynne Davies, B.Sc. (*Wales*).
Johannes Adam De Kock, B.Sc. (*Cape Town*).
John Anthony Dempsey, B.Eng. (*Liverpool*).
Donald Haworth Dickinson, B.Sc. (*Manchester*).
Arthur Dickson.
Leo Direktor, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.
Aubrey Keith Dugdale, B.Sc. (*Manchester*).
Bryan Frank Edbrooke, B.Sc. (*Bristol*).
Brian James Eley, B.Sc. (*Wales*).
Richard Desmond Elliott, B.Sc. (Eng.) (*Lond.*).
Hyacinth Macaulay Ernest, B.Sc. (Eng.) (*Lond.*).
Hugh Glanville Evans, B.Sc. (*Wales*).
Donald Price Ferguson, B.Eng. (*Liverpool*).
James Charles Semark Ferguson, B.Sc. (*St. Andrews*).
Jeffrey Powell Finch-Gasson, B.Sc. (Eng.) (*Lond.*).
Gordon Garvin Finlay, B.Sc. (*Belfast*).
Frederick William Fox, B.A. (*Cantab.*).
Norman Froggatt.
Cecil Leonard Frost.
Peter Garbett, B.A. (*Cantab.*).
Ian MacInnes Gardner, B.Sc. (*Glas.*).
John Edward Gardner.
James Gault, B.Sc. (*Belfast*).
Montague Gaynor, B.Sc. (Eng.) (*Lond.*).
John Hartt Gibson, B.Sc. (Eng.) (*Lond.*).
Frank Montagu Lucas Glaysher.
Ronald Richards Goodwin, B.Sc. (Eng.) (*Lond.*).
Jack Grady.
James Ballantyne Grant, B.Sc.(*Edin.*).
Frederick William Gregory, B.Eng. (*Sheffield*), Stud. I.C.E.
Brian Hall, Stud. I.C.E.
Douglas Edward Hall, B.Sc.Tech. (*Manchester*).
Walter Dixon Handley.
Frederick Joseph Hanley, B.E. (*National*).
Barry Hardcastle, B.Sc. (*Edin.*).
Michael Derek Heather Hayes, B.Eng. (*Liverpool*).
John Blackwell Hellis.
Peter Helliwell, B.Sc. (Eng.) (*Lond.*).
John Clifford Higgins.
Geoffrey Hills.
Peter Holden Hilton, B.A. (*Cantab.*).
Derek Montagu Joy Hogan, B.Sc. (Eng.) (*Lond.*).
David Joseph Pascoe Hollingworth, B.Sc. (*Birmingham*).
Charles Edwyn Geoffrey Holt.
James Hopkinson, B.Sc. (Eng.) (*Lond.*).
David Noel Horgan, B.E. (*National*).
Thomas Gilmour Howie, B.Sc. (*Glas.*).
Donald Sydney Hudspeth, B.Sc. (Eng.) (*Lond.*).
Michael George Hurst, B.Sc. (Eng.) (*Lond.*).
Julius Kenneth Beville Hutchinson.

Anthony Frederick Inderwick, B.Sc. (Eng.) (*Lond.*).
John Edward Jagger, B.Sc. (Eng.) (*Lond.*).
Satyasena Wickramasingha Jayasekera, B.Sc. (Eng.) (*Lond.*).
Malcolm Donald Jeeps, M.A. (*Cantab.*).
Kenneth Richard Jenkins, B.Sc. (*Wales*).
Alan Alexander Katz.
John Biggar Keatinge, B.Sc. (*Glas.*).
Christopher Young Kerr, B.Sc. (*Glas.*).
Eric Hedley Kerry, B.Sc. (*Manchester*).
Peter John Kershaw, B.Sc. (Eng.) (*Lond.*).
Kit Soon Khong, B.A. (*Cantab.*).
William Arthur Kirby, B.Sc. (Eng.) (*Lond.*).
Noel Stuart Kirkwood, B.E. (*New Zealand*).
John Gordon Knibb, B.Eng. (*Liverpool*).
John Romans Lake, B.Sc. (*Edin.*).
John James Langan, B.Eng. (*Liverpool*).
Kenneth Manley Ledson, B.Sc. (*Manchester*).
Edward Stewart Edmund Lee.
John Anthony Brian Lee.
Thomas James Colin Lewis, B.Sc. (*Birmingham*).
Edward Dickson Lindesay, B.Sc. (*Glas.*).
Michael Edward Raymund Little, B.A. (*Cantab.*).
Keith Haydn Livesey, M.Sc. (*Leeds*).
Cyril Llanwarne, B.Sc.Tech. (*Manchester*).
Peter Barrie Snow Lock, B.Sc. (Eng.) (*Lond.*).
John Leslie Venables Lomas, B.Sc. Tech. (*Manchester*).
Kenneth John Ludeman, B.Sc. (Eng.) (*Lond.*).
Francis Edwin McCallion, B.Sc. (*Glas.*).
Alexander Boyd McCallum, B.Sc. (*Edin.*).
Donald Malcolm McDowell, B.Sc. (*St. Andrews*).
Ian Howard McFarlane, B.Sc. (Eng.) (*Lond.*).
Bernard Kevin McGrath, B.Sc. (Eng.) (*Lond.*).
Donald Crowther McKay, B.Sc. (*Wales*).
David Gemmell McKinlay, B.Sc. (*Glas.*).
James Garvin McKinney, B.Sc. (*Belfast*).
Daniel McLean, B.Sc. (*Glas.*).
John Alexander Stuart McLeod, B.Sc. (Eng.) (*Lond.*).
David William Manton, B.Sc. (Eng.) (*Lond.*).
Kumpati Balasubrah Manyam, M.Sc. Tech. (*Manchester*), B.E. (*Madras*).
Leonard Henry Painter Marsh.
John Ellery Martin.
John Edgar Massey, B.Sc. (*Manchester*).
Arthur James Dudley Mathews.
Ronald Duncan May, B.Eng. (*Sheffield*).
David Arthur Hinds Maysey, B.Sc. (*Wales*).
Mahmood Miakhan, M.Sc.Tech. (*Manchester*), B.E. (*Madras*).
Frank Micklethwaite, B.Eng. (*Sheffield*).
James Angus Millar, B.Sc. (*Belfast*), Stud. I.C.E.
Gordon Manchester Mills, B.Sc.Tech. (*Manchester*).
Michael Minter.
John Sidney Mitchell, B.Sc. (*Manchester*).
Norman Lowe Mitchell, B.Sc. (*St. Andrews*).
Ancel Donald Morgan, B.Sc. (*Leeds*).
Michael John Morgan, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.
William Bain Morgan, B.Sc. (*Belfast*).
Raymond Stanley Morris.
Robert William Morris, B.A. (*Cantab.*).
Sholto Douglas Patrick Morton.
John Leslie Morton-Smith, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.
Ian Thomas Munro, B.Sc. (Eng.) (*Lond.*).
Gamil Abdul Nabi, M.Sc. (Eng.) (*Lond.*), B.Sc. (*Cairo*).
Shiw Sahai Naraine.
John Nicholas, B.Sc. (*Birmingham*).
Alan Geoffrey Norman, B.Sc. (*Nottingham*).
Henry McCann O'Hanlon, B.Sc. (*Glas.*).
Diarmuid Michael O'Hegarty, B.E., B.Sc. (*National*).
David John Palmer, M.A. (*Oxon.*).
Kenneth Walter Pearce.
John Adrian Percival, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.
Raymond Jeffery Phillips.
James Alfred Philpott, B.Sc. (Eng.) (*Lond.*).
John Marks Plowman, Ph.D., B.Sc. (Eng.) (*Lond.*).
Geoffrey Arthur Potter, B.Sc. (*Birmingham*).
Victor Thomas Poulton.
David Solomon Powell, B.Sc. (Eng.) (*Lond.*).
John Charles-Power, M.A. (*Cantab.*).
George Ramage, B.Sc. (*Glas.*).
Terence Edward Rampling, B.Sc. (Eng.) (*Lond.*).
Charles William Anthony Reay, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.

The Hon. Nicholas Ridley, B.A. (*Oxon.*).
Arthur Rushton Rigby, B.Sc. (*Manchester*).
Basil Albert Ring.
Raphael Foner Rish, B.Sc. (Eng.) (*Lond.*).
Peter Yeats Robb, B.Sc. (*Aberdeen*).
William Nelson Robertson, B.Sc. (*Edin.*).
Kenneth Edward Robinson, B.Sc. (Eng.) (*Lond.*), Stud. I.C.E.
Robert Kenneth Ivor Roe, B.A., B.A.I. (*Dublin*).
John Leonard Maxfield Rogers, B.A. (*Cantab.*).
Robert George Russon, Stud. I.C.E.
Patrick Francis Ryan, B.E. (*National*).
John Alexander Sandover, B.Sc. (Eng.) (*Lond.*).
Nagalingam Saravanapavananthan, B.Sc. (Eng.) (*Lond.*).
Kenneth John Harley Saxton, B.A. (*Cantab.*).
Dov Schreier, B.Sc. (*Witwatersrand*).
John William Scott.
James Lawrie Seale, B.A. (*Cantab.*).
William Gordon Sharrock, B.Sc. (*Manchester*).
Bryan Edward Shipp.
George William Charles Shuttle, B.Sc. (Eng.) (*Lond.*).
John Edward Simpson.
Keith Stanley Smith, B.Sc. (Eng.) (*Lond.*).
Kennet Robert Smyth-Osbourne, B.A. (*Cantab.*).
Jack Stein, B.Sc. (*Edin.*).
Terence Litchfield Stiles.
John Gordon Stuart, B.Sc. (*Wales*).
Robert Charles Styles.
Kathiravelu Sundaranadarasa, B.Sc. (Eng.) (*Lond.*).
Thambipillai Suppiramaniam.
Harry Sutcliffe, M.Sc. (*California*), B.Sc. (*Manchester*).
Benjamin Herbert Charles Sutton, B.Sc. (Eng.) (*Lond.*).
Dennis Taylor, B.Sc. Tech. (*Manchester*).
Kanapathipillai Thilaganathan, B.Sc. (Eng.) (*Lond.*).
David Alan Thom.
David Morris Thomas.
Leslie Henry Thompson.
John Thorp.
Robert Errington Tibbs.
Richard William Trask, B.Sc. (*Edin.*).
John Deacon Trotman, B.Sc. (*Bristol*).
David John Tuckwell, B.Sc. (Eng.) (*Lond.*).
Edwin William Tunna, B.Sc. (*Manchester*).
Leonard Andrew Bruce Turner.
Ivar Leslie Twelves.
Walter Sydney Twelvetrees.
Samuel Patrick Chukwuma Ugona, B.Eng. (*Sheffield*).
Per Helge Ulstad, B.Sc. (*Birmingham*).
Peter Walker.
Gordon Crichton Wallace, B.Eng. (*Liverpool*), Stud. I.C.E.
Charles Henry Oliver Waters, B.Sc. (*Durham*).
Peter Michael Weatherhead, B.Sc. (*Bristol*).
Bernard William Webster, B.Sc.Tech. (*Manchester*).
Lawrence West, B.Sc. (Eng.) (*Lond.*).
George Deryk Wilkinson, B.Sc. (*Durham*).
Michael Wilkinson, B.Eng. (*Sheffield*).
Idwal Williams.
John Tate Williams, B.Sc. (Eng.) (*Lond.*).
Richard Evan Williams, B.Sc. (*Durham*).
Bernard Arthur Wilson, B.Sc. (Eng.) (*Lond.*).
James Gordon Wiltshire, B.A. (*Cantab.*).
Godfrey Frederick Guy Worraker, B.Sc. (*Bristol*).
William Ronald Macdonald Wyatt, B.Sc. (*Glas.*).

JAMES FORREST LECTURE, 1952

The President said that the James Forrest Lecture had been founded in 1891 in honour of James Forrest, who had been Secretary of the Institution from 1859 to 1896, and Honorary Secretary from 1896 until his death in 1917. The Lecture about to be delivered was the fifty-eighth of the series.

The Lecturer, Mr Harding, had had a wide practical experience of soil mechanics and was taking as his subject " Progress of the Science of Soil Mechanics in the Past Decade." He had no doubt that in the Lecture Mr Harding would indicate what had been accomplished in that field since Professor Terzaghi delivered the James Forrest Lecture on the subject a little more than a decade ago, in 1939.

The Lecture was then delivered.

" The Progress of the Science of Soil Mechanics in the Past Decade "

by

Harold John Boyer Harding, B.Sc.(Eng.), M.I.C.E.

THE subject for the 58th James Forrest Lecture was chosen by the Council. It is traditional to recall that the first Lecture was delivered in 1891 by Dr William Anderson, and that the Council prescribed the theme of these lectures as " the interdependence of abstract science and engineering."

In 1939, Dr Terzaghi gave the 45th Lecture [1] under the title of " Soil Mechanics—a New Chapter in Engineering Science," and that was the signal for a great increase in British interest and learning on the subject. The Council of the Institution had hoped that Dr Terzaghi could have returned to Great Britain and given a sequel describing developments since that Lecture, but to our loss he has not been able to do so, and the Council has honoured me by an invitation to speak on this subject.

I have interpreted the title provided for me rather freely, and shall deal with the historical aspect in order to leave a record for future readers of a colourful period of development in our great profession, which continued undeterred and even stimulated, through the greatest conflict in history, and greatly encouraged and aided by this, our own Institution.

The subject of Soil Mechanics has been described as forming one of the three major disciplines of civil engineering, the others being the theories of structures and of hydraulics.

[1] The references are given on p. 679.

To me, Soil Mechanics consists of :

the investigation of soil conditions as they exist ;
the classification and testing of properties of what is found ; and
the application of various theories and practices which have been checked against experience, and which explain and foretell soil behaviour.

The subject was officially christened at the First International Conference on Soil Mechanics and Foundation Engineering at Harvard University in 1936. It covers not only soils, but all the materials composing the earth's crust on which man must construct, and under which, at times, he has to live. The subject is old, but has advanced more slowly than many another science. This is due to difficulties inherent in its nature.

The general conditions under which the science of civil engineering is carried out have few parallels with any other calling. The atomic scientist causes our wonder and amazement by isolating his various materials to their simplest form and then causing them to obey his bidding in a fearsome and awe-inspiring manner. The metallurgist analyses the natural material of his study and moulds it to the form he requires. The mechanical inventor continually improves his devices, improving on his man-made machine under conditions of his own choosing.

The civil engineer must go to where his problem lies ; he must start from nothing and he must take his material and the medium in which he works exactly as Nature has laid it down in all her innumerable varieties and incalculable moods. So the civil engineer's work in the field must, by its very nature, be primitive, and from its vast size be the concern of a number of men. His machines must go to the work, and not the materials to his machines ; they, of necessity, are generally powerful, brutish, and primitive.

Among theoretical soil studies the first to develop was Earth Pressure Theory, as it applied to banks and walls. The French carried out the pioneer work under the spur of military necessity, commencing with Vauban (1633–1707). Coulomb, whose famous work was published in 1766, was a Colonel of Engineers. Later, with the growth of civil engineering works, the subject was developed by Poncelet and Collin in 1840 and 1846.

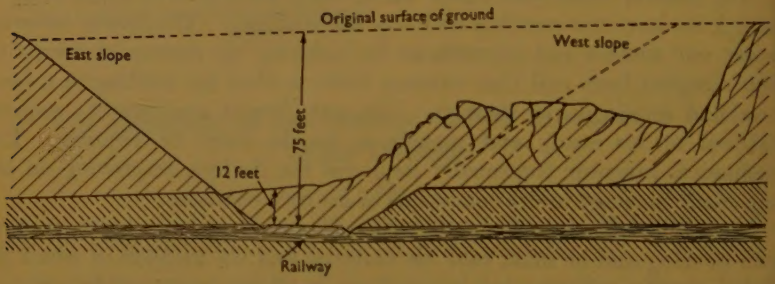
During the canal and railway ages in Great Britain the rate of expansion of works in tunnels, embankments, and cuttings in great varieties of soil had possibly never been exceeded. The experience gained seems to have been handed down almost privately from master to pupil, because of the system of training which prevailed at that time, but in 1844, C. H. Gregory presented Paper No. 668 to the Institution,[2] giving an account of some slips in the London Clay on the line of the London and Croydon Railway (*Fig. 1*). In his Paper, and in the discussion which

followed for several evenings, experiences were recorded and theories provided of their causes.

Throughout the publications of the Institution occasional Papers call attention to soil problems and record experience, but they occur as isolated phenomena and received little recognition in their time.

In 1857, Professor Rankine published his Paper [3] to the Royal Society on "The Stability of Loose Earth," and he repeated the substance of his theory of conjugate stress and its application to earth pressure in simplified form in his text books. Rankine concluded the section on "Earth Pressure" in his Manual of Civil Engineering with these words: "There is a mathematical theory of the combined action of friction and adhesion in earth; but for want of precise experimental data its practical utility is

Fig. 1



POSITION OF THE SLIP IN THE NEW CROSS CUTTING, 2ND NOVEMBER, 1841

doubtful." Some of Rankine's disciples did not fully understand his assumptions and, largely because of their faulty teaching, serious fallacies were embodied in the practical application of his theories; it has even been said that Rankine's work did more to hinder developments of earth pressure theory, and so of soil mechanics, than to help it forward. Truly great men have more to fear from their disciples than from their opponents!

Rankine's theoretical approach provoked a reaction when, in 1881, Sir Benjamin Baker—exemplar of common sense—delivered his famous Paper [4] before the Institution on "The Actual Lateral Pressure of Earthwork." It has been thought by some who have not studied the Paper that Sir Benjamin was attacking the scientific approach, but that was not so. He complained of the want of experimental data, so that individual judgement had to be exercised in each instance without that aid from careful experimental investigation and observation which at that time was the root of progress in almost every other branch of engineering.

The bulk of the then existing literature, he said, consisted of arithmetical changes rung on a century-old theory. That provoked Professor Boussinesq to reply in correspondence by ringing no less than twelve pages

of mathematical changes without producing much of practical value at the end of it.

Baker went on to say that it is far more simple and direct to assume the thickness of the wall than to derive the latter from equations based on a number of uncertain assumptions.

" This being so," he continued, " it has often struck the Author that the numerous published tables giving the calculated required thickness of retaining walls to three places of decimals, rely upon exactly the same scientific basis, and have the same practical value, as the weather forecasts for the year in Old Moore's Almanack."

The attention of engineers had been focused upon soil problems, but it was diverted into mathematical theory, because that was the prevailing fashion of the period, and possibly retarded the development of sampling and testing techniques. It is the purpose of this Lecture to show that we have progressed a long way in the last decade towards providing data so that calculations can be made which are not based wholly upon assumptions.

In 1915, A. L. Bell presented to the Institution his Paper [5] on " The Lateral Pressure and Resistance of Clay, and the Supporting Power of Clay Foundations," and he followed it up by his Paper to the Engineering Conference in 1921 on " The Bearing Power of Soils." Bell's formula, which he then published, has become accepted throughout the world. This British pioneer was far ahead of his contemporaries, and it was 20 years before his work bore fruit in his own country.

In 1933, a Soil Physics Section was started in the Building Research Station to continue the research which Professor Jenkin had started at Oxford on lateral earth pressures. The British Association Committee on Earth Pressures voted £10 towards that work and later, in 1937, the Institution of Civil Engineers Research Committee on Earth Pressures took over responsibility. That Committee later became the Soil Mechanics and Foundations Committee of the Research Committee of the Institution. They reviewed the work in progress in laboratories in Great Britain and were eventually responsible for publishing in 1950 the Bibliography on Soil Mechanics,[6] which contains 3,300 references.

The rapid development of the work in Great Britain was thus greatly helped by this Institution. The Building Research Station also undertook research in soil problems relating to roads on behalf of the then newly formed Roads Research Board, and at once began to follow the lines along which soil mechanics was developing abroad.

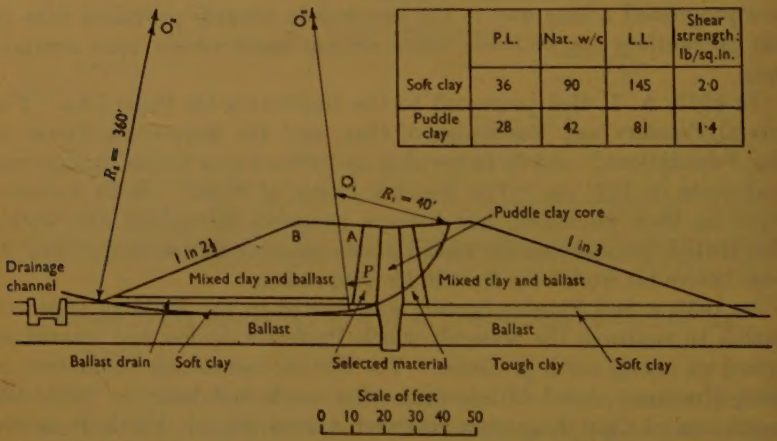
In June 1936, there appeared in the Journal a note on " The Shearing Resistance of Soils " in the Report of the Institution Research Committee.

In 1936, Terzaghi made the second essential step after Coulomb by calling attention to the influence of small movements on the distribution of earth pressure; also to the importance of understanding the soil properties, and to the development of methods of measuring them.[7]

The words " Soil Mechanics " first appear in the Index of the Institution's publications when a report appeared, in the Journal for November 1936, of the First International Conference on Soil Mechanics and Foundation Engineering held at Harvard, U.S.A. Two hundred delegates had assembled from twenty nations, which shows how much interest had been stimulated in other countries. Only one British delegate attended and two British Papers were presented.[8, 9]

The Second International Conference on Soil Mechanics and Foundation Engineering was held at The Hague in 1948. There were 596 participants from thirty-eight countries. There were sixty-nine British delegates and sixty-seven British Papers were published. Thirteen British laboratories presented reports on their organization and equipment.

Fig. 2



	P.L.	Nat. w/c	L.L.	Shear strength: lb/sq.in.
Soft clay	36	90	145	2·0
Puddle clay	28	42	81	1·4

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DIAGRAM OF SLIP AT CHINGFORD RESERVOIR

Certain works often act as milestones in progress, and are the cause of sudden development. In 1937, the partly completed earth bank of Chingford Reservoir failed by a slip in the soft alluvial clay which lay between the bank and the river gravel (*Fig. 2*). This was the first occasion on which the heavy excavation plant of tractor and scraper had been used in Great Britain, with its great acceleration in the rate of bank building.

The team of young enthusiasts at the Building Research Station seized the opportunity to leave small-scale laboratory research and carry out a full-scale investigation into this unfortunate but important occurrence. They produced diagrams explaining the cause of the slip, which have since been published in a Paper.[10] At the same time Dr Terzaghi was called in for his opinion ; it completely vindicated the Building Research Station's

solution, which previously had been viewed with some scepticism by older members of the profession. He also produced a revised design for the bank (*Fig. 3*), which was to be based upon measured values of the strength of the soils as the work proceeded.

This occurrence, and the confidence which it gave to those interested in the subject, provided an impetus to the development of soil mechanics. Universities, Railway Companies, and Government Departments, and even private firms sent selected engineers to the Station for instructional purposes. The Station was then overloaded with soils work, and these men were put to work to assist the permanent staff, so that they were grounded in the subject by that valuable method—practical experience.

This rapid increase in interest reached a new peak when the Institution invited Dr Terzaghi to deliver the James Forrest Lecture in 1939, which attracted an audience who could absorb the implications of his Lecture. Those who were fortunate enough to hear him deliver his Lecture in this room will recall the impact of his strong, but curiously impersonal personality, and the thrill and feeling of real excitement which he produced in a way not often felt in these surroundings. At that time in Great Britain the subject was being studied by only a handful of people, and on reading his Lecture again it is astonishing to see how much Dr Terzaghi covered and how far in advance of his contemporaries he was in 1939. In fact, the dogma has not increased. The disciples have become far more numerous.

These are the principal sections into which he divided his lecture, compared with subsequent developments in each case :

The Pressure of Earth on Lateral Supports

He showed that in the case of sands the contradiction between theory and practice had, in the preceding 20 years, ceased to exist, but in the case of clays he said that, since the complete mobilization of shearing resistance was not realized, problems dealing with the pressure exerted by clays under field conditions were beyond the scope of earth pressure theories in general.

" In our future treatment of these problems," he said, " we must choose between maintaining our present state of ignorance, and acquiring a stock of reliable experience by systematic measurements and identification tests in the field."

The stock of reliable experience for which he asked has been acquired to a considerable extent over the last decade by systematic measurements.

Owing to the enlightened attitude which has developed, facilities have been given on a number of large-scale works for the recording of actual stress measurements in cofferdams, trenches, and tunnels (*Fig. 4**).

It can now be maintained that the pressure on trench linings and the like can be predicted with sufficient accuracy for practical purposes,

* *Figs 4, 7* to *12, 14,* and *15* are photographs and appear between pp. 670 and 671.

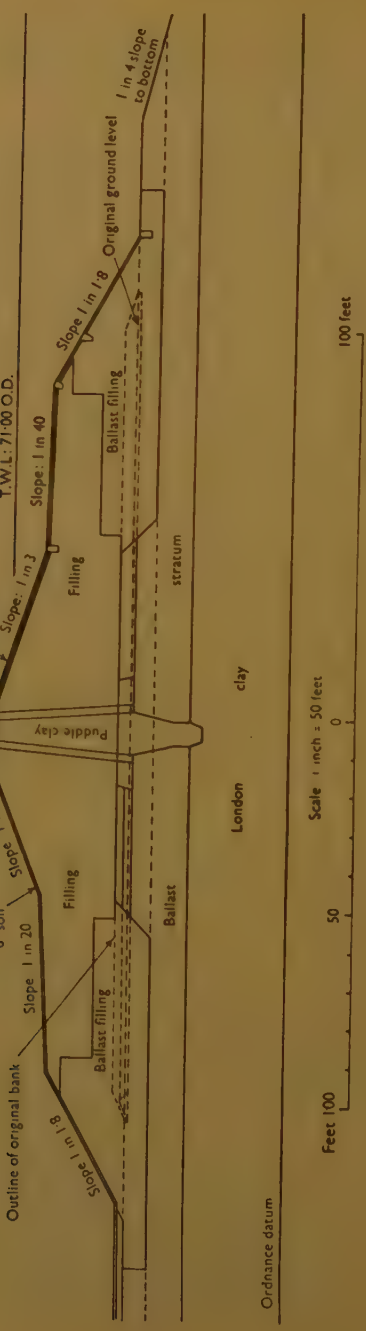


Fig. 3

REVISED DESIGN OF BANK AT CHINGFORD RESERVOIR

from a knowledge of the shear-strength characteristics of the soils, as many full-scale measurements have shown.

Stability of Slopes and Failure of Dams Due to Piping

Terzaghi described the work of Fellenius and Olsson and the so-called "Swedish Method" of determining the degree of stability depending upon tested properties of the soils. He emphasized the importance of the effect of porosity and compaction, and the nature of flow slides, and related those problems of seepage forces with the failure of dams due to piping. Dr Terzaghi showed that Bligh's ratio,[11] which was of outstanding importance in the state of knowledge when it was published in 1910, was really faulty in certain assumptions, and he described his own work on horizontal inverted filters as a correction to possible piping.

Since 1939, many failures have been analysed, especially in clay soils. In Britain the term $\phi = 0$ has come into common usage to describe the much simplified method applied to cohesive materials in which the shear strength is assumed to be half the unconfined compression strength. Simple though it is, the portable compression apparatus first devised in Britain and now in use all over the world is a notable advance in field technique.

In a number of cases where slips have started it has been possible to calculate the strength of the soil from a knowledge of the mechanism involved, and so design remedial measures which were based on a low but adequate factor of safety.

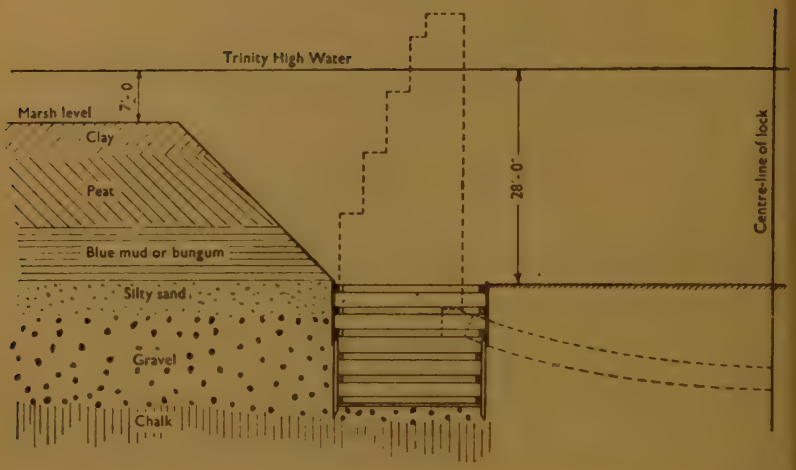
Figs 5 is a drawing, by courtesy of the Port of London Authority, of the Gallions Entrance to the Royal Albert Dock, constructed in 1885. The section in *Fig. 5* (*a*) shows the timbering and excavation originally proposed according to borings taken. *Fig. 5* (*b*) shows a slip which occurred during excavation. The circular nature of such slips was recorded in those days, but for lack of means of testing for strength the borings could not be put to proper use. *Fig. 5* (*c*) shows the work as carried out, with a greatly reduced amount of open excavation.

The Gallions Entrance is now being reconstructed, and it was necessary to calculate the stability of the wall when the invert was cut out. That was done by means of strength tests from fresh borings, but the slip in *Fig. 5* (*b*) was used as a means of calculating the shear strength of the soft material behind the wall as a check of the wall's stability.

Much progress has been made in the solution of piping problems by the development of improved methods of flow-net analysis. These include the theoretical approach, such as the application of relaxation methods to flow-net problems, and on the practical side laboratory model-technique has been improved with the aid of simplified electrical methods, but all these theories refer to homogeneous materials.

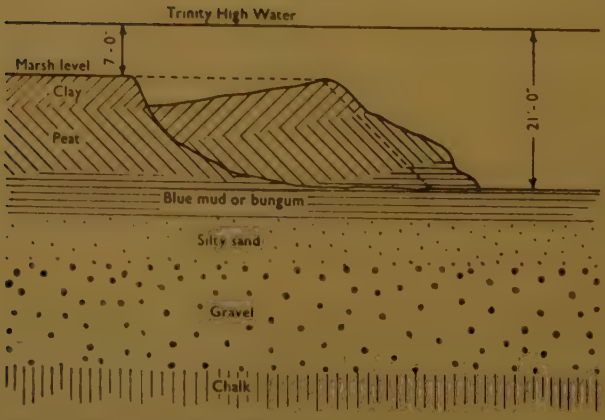
We must always remember the great practical importance of even

Fig. 5 (a)



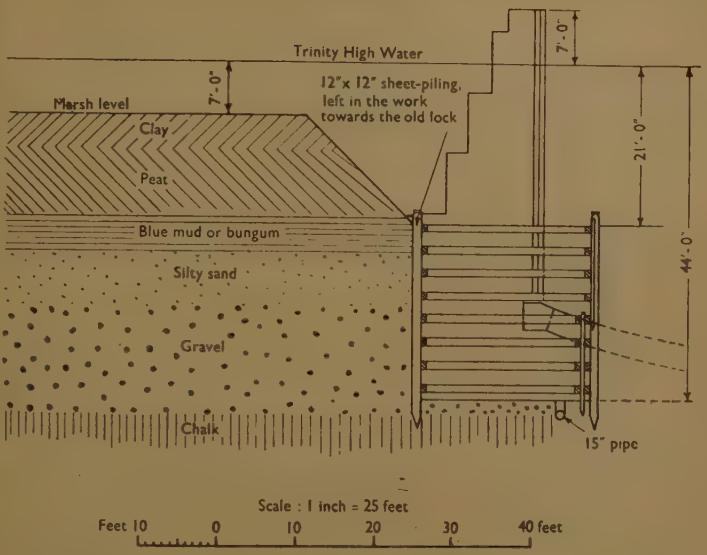
SECTION OF TIMBERING AND EXCAVATION AS PROPOSED (ACCORDING TO BORINGS TAKEN)

Fig. 5 (b)



SECTION OF EXCAVATION SHOWING SLIP AND THE ACTUAL FORMATION OF STRATA

Fig. 5 (c)



SECTION SHOWING EXCAVATION AND METHOD OF TIMBERING USED IN CARRYING OUT THE WORK

ROYAL ALBERT DOCK, NEW ENTRANCE, 1885.
FOUNDATIONS FOR DOCK WALLS

small departures from uniformity, which occur so constantly in nature. Theoretical solutions must be balanced by practical experience.

In several recent large excavations in which analyses have been made by these methods, their limitations have been proved on excavation by the degree to which the ground varied from the ideal material which had to be assumed. A certain degree of empirical correction will always be necessary in such cases.

The Consolidation of Clays and its Practical Consequences

Dr Terzaghi pointed out that the older text books on engineering allowed the time element to pass unnoticed, and he stressed how important the time element is in the case of consolidation of soft clays, especially if water is withdrawn from under the clay layer. Although, since then, considerable work has been done on the theory of consolidation, the simple theory propounded by Terzaghi still proves adequate for most practical problems. The theoretical aspect has not changed, but understanding of its importance has become widespread and led to an interesting Paper [12] on "The Settlement of London Clay through Drainage of the Chalk."

Since the publication of the Theory of Clay Consolidation,[13] many cracks have been noted and reported upon which previously had passed unexplained, and sometimes unnoticed, as in the case of the tower of Westminster Cathedral. It has become a commonplace to investigate and forecast settlements from consolidation tests, and engineers now design for settlements, where 20 years ago they would have been horrified to contemplate such settlements, even though they constantly occurred.

Settlement of Footings and of Raft Foundations

Terzaghi, within the limits of space in his Lecture, referred only to consolidation of material below the footing. Another criterion exists—that there should be an adequate factor of safety against shear failure. Several failures of foundations on soft clay have been analysed in Great Britain, again using the $\phi = 0$ method, among them those of oil tanks—a problem of increasing importance—and now, from measurements of shear strength, it is possible to predict a bearing pressure which will be safe against the shear failure of the soil.

Settlement of Pile Foundations

Dr Terzaghi impressed his listeners by saying that the widespread opinion that a favourable outcome of a loading test on an individual pile excluded the possibility of serious settlements of an entire pile foundation, was among the most incomprehensible and detrimental prejudices in the field of civil engineering. This lesson has been learnt by some, and several large power stations have recently been constructed where foundation piles have been driven through soft compressible layers,

even at the expense of difficult pile driving, in order to avoid dangerous and differential settlements of the whole structure.

It is also at last beginning to be realized that a piling formula is not an accurate assessment of pile-bearing capacity, and that only actual loading tests to failure give true results. The Research Committee has asked for data of tests to be sent to them, and it is indicative of how few tests are made that the response so far has been so meagre.

The note sounded throughout the Second Conference was the need for more recorded experience in every field of the subject. This especially applies to piles and pile driving, because the reliance on formulae only was severely attacked. We now know that there are unexplored phenomena in the effect of driving piles into undisturbed clay on the compressibility of clay located between the piles, and that negative shear friction can develop which drags the pile down and forces its point deeper into the sub-stratum.

In sands the pore-water pressure and the effect on them of driving can have varied results. Pile driving in layers of loosely packed fine sand causes excess pore-water pressures, and so a temporary drop in penetration resistance of the soil. After some time, as soon as the excess pressure has gone, penetration resistance rises again. So that pile-driving formulae based on a certain penetration per blow cannot provide a measure for the bearing capacity of piles driven into loose sand layers.

This hot desire to find a formula and apply it to all sorts of piling is due to a wish to have a ready-made answer in order to avoid the work and cost of loading tests, which are always expensive. Such a formula must remain most approximate, unless correlated to actual tests on the site. In Great Britain, piles are seldom driven into one uniform type of ground. There is still work to be done in examining, by means of undisturbed samples, the alteration to strata between piles after driving.

Sampling and Testing

In the last two decades in Great Britain the advance made in sampling and testing, upon which all applications of theory and practice depend, has been astonishing. The techniques of Soil Mechanics have been extended to cover soft rocks which lie beyond the stiff soil range, and, in fact, the whole geological field as applied to construction work, linking with geology in a quantitative manner.

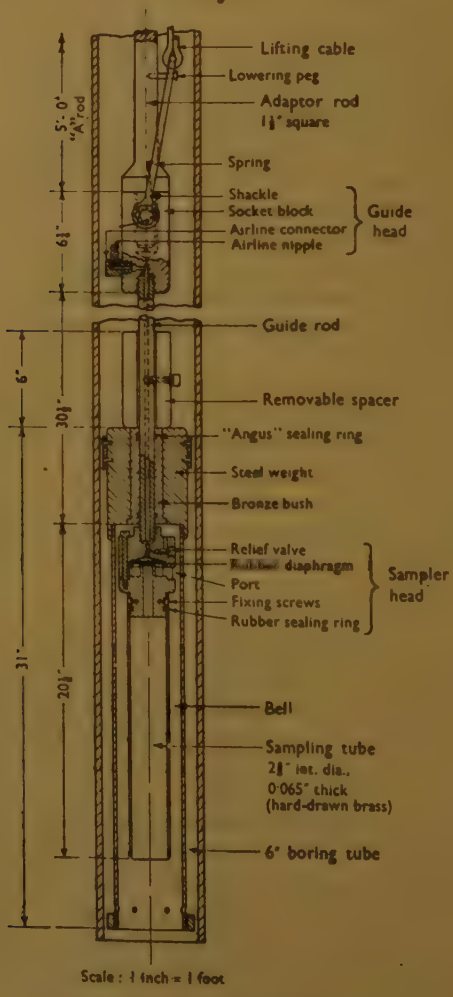
Before the First Conference, site investigation had been conducted in a crude way. In the United States of America, wash borings were much used, with soil identified by the washings from the wash water, regardless from what level it originated. In Great Britain, samples were left to dry out in wooden boxes, so that ultimately they bore no resemblance to their original form. Now, in 1952, thin-walled sample tubes are standard practice, with samples kept in air-tight containers and

carefully re-described, according to Codes of Practice, to check the description of the boring foreman.

In Great Britain, which contains more varieties in geological deposits than any other similar area, it has been found expedient to try to develop a simple form of sampling which is most likely to succeed, whatever conditions are revealed.

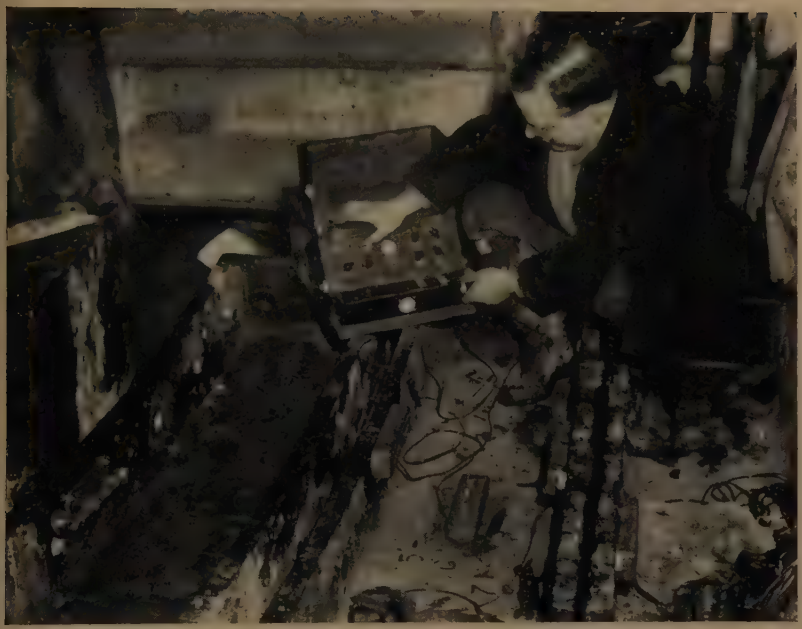
The problem of sampling sands was not really solved until the development of the compressed-air sampler (*Fig. 6*), which is the most important sampling advance in the last decade.

Fig. 6



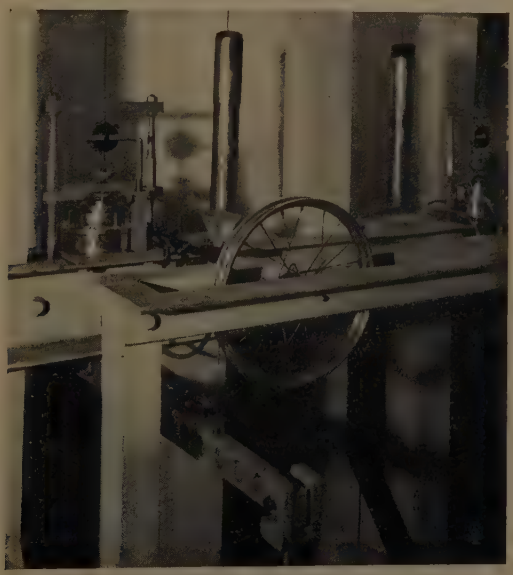
COMPRESSED-AIR SAND SAMPLER

Fig. 4



(*Crown Copyright reserved. Published by permission of the Director of Building Research*)
ACOUSTIC WIRE STRAIN GAUGE AND TESTING APPARATUS FOR 25-FOOT-DIAMETER CAST-IRON TUNNEL LINING IN CLAY

Fig. 7



CONSOLIDATION PRESS, 1938

Fig. 8

BATTERY OF CONSOLIDATION PRESSES, 1952

Fig. 9

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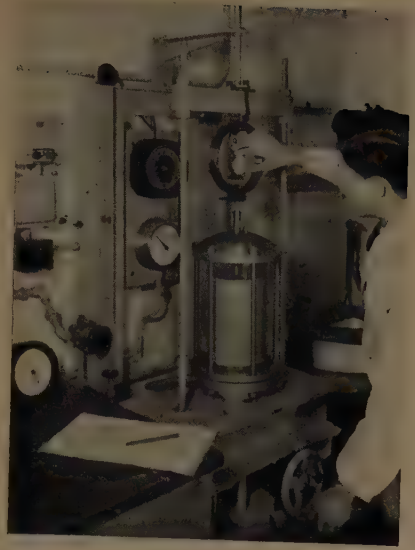
EARLY TRIAXIAL TEST APPARATUS, 1939

Fig. 10



ELECTRICALLY-DRIVEN TRIAXIAL TEST APPARATUS, FOR 4-INCH-DIAMETER SAMPLES, 1952

Fig. 11

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RING SHEAR APPARATUS, 1934

Fig. 12

ELECTRICALLY-DRIVEN BOX SHEAR APPARATUS, 12 INCHES SQUARE, 1952

Fig. 14

GEOPHYSICAL SURVEY OF RESISTIVITY METHOD, FIJI

Fig. 15

GEOPHYSICAL SURVEY BY SEISMIC REFRACTION METHOD, PAKISTAN

In 1939, the principal testing machines which had been developed were the shear box, the unconfined compression apparatus, and the consolidation press. These have proved to be the most suitable for British conditions, although in other countries other tests have been developed which are more suitable to their particular problems, which depend upon their local geology.

By 1952, the development in laboratory apparatus has been more in the direction of perfecting these machines and improving them rather than developing any revolutionary tests. Early apparatus had limitations if stones occurred in samples, since small-scale test specimens could not be prepared. This has been overcome by the development of the tri-axial apparatus in which a complete core of 4 inches diameter can be tested, and by shear boxes with a surface area of 1 square foot (*Figs 7–12*).

Methods of in-situ testing have been developed to a greater extent, the most important being the vane test for use in soft and very sensitive clays and silts (*Figs 13*). This has now got beyond the more primitive form, and the mechanism is being constructed to instrument-makers' standards.

Several speakers in recent discussions mentioned vane-test results as giving higher strengths in soft clays of a very sensitive nature than given by testing " undisturbed " samples. They appeared to wish to deride the latter. As some will foresee, as the inversion of the first criticism, already there are those demanding vane tests in a stiff fissured clay, which is absurd. This is an example of the lack of thought and appreciation which often causes such criticism.

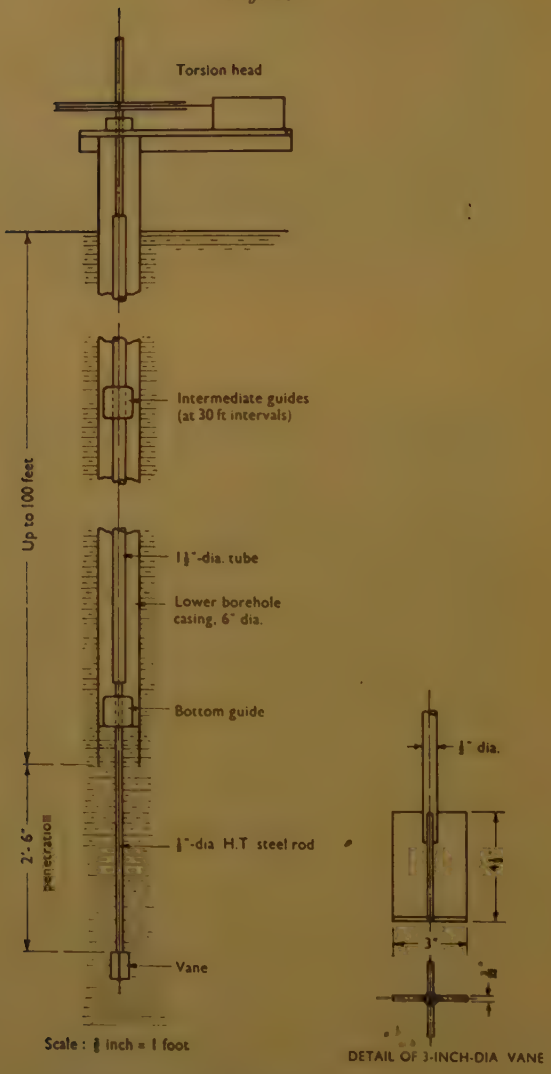
The vane test was designed purposely to try to overcome the sensitivity of certain soft clays, which made sampling difficult. In non-sensitive clays the results given are the same. It should be obvious that to obtain 100-per-cent non-disturbance by driving any sampler or even a vane into a very sensitive clay is not possible. The title " undisturbed " is understood by soil mechanics workers to mean as undisturbed as humanly possible, as distinct from samples disturbed by boring tools. In soft sensitive clays a sample will be weaker than the original material, an error which is on the side of safety.

Some construction methods, such as pile driving or mechanical excavation, may reduce the shear strength of sensitive clays to that measured by the normal sampling and testing procedure. We need to find the real strength before, during, and after constructional operations.

It is interesting to speculate on the mental process which prefers a test which gives a higher strength, regardless of which result is nearer the truth !

Diamond drilling has long been practised in mining, but before 1946 was rarely used in civil engineering site investigation. This is due to

Figs 13



NOTE:-
The Torsion System runs in ball bearings throughout, those in guides being sealed in grease against the entry of water or soil

VANE APPARATUS

the long popularity of chilled shot boring in the softer English strata producing large diameter cores, but the very hard schists encountered in the Scottish Hydro-Electric works called for extensive diamond drilling. It is symptomatic that at first some engineers were slow to appreciate the value of raking holes for finding faults under rivers, or other purposes, as they were accustomed to borings being vertical where chilled shot was used. Diamond drilling is now extensively used to prove bedrock and so to avoid the mistaking of enormous boulders for bedrock.

The Paper [14] on the famous Silent Valley Reservoir episode is important to us all.

The scope of geophysical surveying, which forms a link in Soil Mechanics, has also been increased, and both its benefits and limitations are more widely appreciated.

Resistivity methods were first hailed as a cheap way of getting quick results. But in some cases where these were not related to borings, wrong deductions were made and so the method fell for a period into disrepute.

Fig. 14 shows a resistivity survey being carried out across a river in Fiji. The ropes are at an equal distance apart. Electrodes were suspended into the water from the ropes, and measurements were made every 20 feet across the river as the electrodes were moved along the ropes.

The seismic method, which at first was considered less suitable in civil engineering, has been used to a greater extent in recent years, and in certain conditions is an improvement over the resistivity method. *Fig. 15* shows a seismic shot being fired in a river in Pakistan, with geophones, or vibration pick-ups, situated on the bank of the river. Several shots were fired across each section of the river to be investigated.

In all these the Code of Practice rightly insists on calibration of readings to actual borehole results, since deductions from the unseen in such a varied field as the subsurface of the earth has led to erroneous results on occasions where desire for economy and over-confidence in deductions has led to false assumptions in design.

In the Second Conference, speaker after speaker emphasized that what we needed was the greatest possible number of carefully recorded observations of practical examples rather than fresh theories. The progress in the last decade has been on these lines and can be seen by the greatly extended facilities for taking samples and testing, and the extent to which these have been used.

In 1937, there were in effect only two laboratories in Great Britain which could undertake soil tests, and these were equipped only for research. In 1951, thirty-four laboratories reported to the British National Committee from Universities, Research Stations, and private enterprise.

When the war started in 1939, the sudden stirring of interest received a momentary setback. However, much earthwork had to be carried out at filling factories, airfields, and war-time constructions, and the Building Research Station quickly became busy in investigations of sites and also

of earthwork and foundation failures as they occurred. No more fertil field of experience could be found than full-scale research in this way.

An informal Discussion Group was formed, which met in this Institutio in 1940–41, but was interrupted by increased enemy activity.

The work of the Site Investigation Codes of Practice Committee starte in 1943 at a time when the interest in the subject was increasing. Variou Papers delivered to the Institution mark the development in Great Britain During the period of the war the Institution of Civil Engineers publishe more than thirty Papers on Soil Mechanics subjects. These Papers wer welcomed by workers in this field. New definitions had to be learnt, an a description of strength tests, which the average engineer could not the readily obtain, made them appear academic at first, but as facilities fo testing multiplied, so the value of these Papers became better appreciated

Since 1939, the Road Research Laboratory has carried out much wor on soil problems, but owing to the post-war road programme bein negligible, they have been prevented from putting their research int practice on a large scale. But Soil Mechanics has been applied to runway to a considerable extent. In 1939, runways in concrete were coming int use in place of grass surfaces, in order to cope with ever-increasing weigh of aircraft and their increased impact loads. The wartime airfield con struction programme was the most intense concentration of one type o construction since the original railways. At first, designs were mad depending upon assessing a thickness of concrete. It was during the wa that soil engineers were able to impress upon the authorities the fact tha the most important factor was the strength of soil supporting the carpet and the need to spread the load by thickness rather than strength wa realized.

Most methods so far in use are empirical and semi-empirical, such a the California Bearing Ratio and plate-bearing tests. But Papers befor this Institution have suggested methods based on measuring the strengt of the soil rather than the strength of the concrete.

Soil stabilization had been developed in the United States as a means o making roads and runways in areas where only the natural soil material were available. This method consists of careful compaction, with th addition of a stabilizer to assist in binding, but primarily to repel absorptio of water which causes softening. The optimum moisture content at whic such material should be compacted became the dominant test. Her again too much was hoped for at first in Great Britain—a predominantl clay country—and engineers sometimes failed to realize that a stabilized soil road consisted of a soil which had been compacted and that this coul not possibly be compared to a first-grade arterial road.

In the summer of 1945, the Institution arranged for a series of fou lectures [15] on Soil Mechanics, which drew big attendances and many copie have been distributed and read. These summed up the previous work i a convenient form. In another Paper,[16] on "Soil Drainage with Par

ticular Reference to Road Engineering," the suggestion was put forward that a Paper on engineering biology would be of importance. The Institution followed up this suggestion and held a four-day conference [17] in September 1948 on " Biology and Civil Engineering." Valuable Papers were read on soil erosion, vegetation for stabilizing slopes, and algae growth and their effects on design, but most important of all, the effect of vegetation on the settlement of roads and structures. These Papers gave striking examples of the effect of roots of trees and vegetation in causing shrinkage failures in the foundations of buildings and roads.

The Codes of Practice Committee on Site Investigation was in constant session during the latter part of the war and until 1951. Quite a large part of this Code (which is permissive and not compulsory) is of an educational or informative kind. This has been criticized by some but welcomed by the majority. The geological portions are of considerable value, and the standard descriptions are based on soil properties and not local names. The Code concentrated upon uniform descriptions and tabulation of soils, according to their properties, and a much-needed division between clays, silts, and sands.

A Paper [18] of great influence was " Particle-size in Silts and Sands," which examined the several rival classifications and came down heavily on the side of the M.I.T. granular sizes. It showed how the divisions in this sizing curve were definitely related to changes in properties and also the engineering expedients applicable to them. This influenced the Committee to accept the M.I.T. System for the Code.

It is as well that such Codes are only permissive, for much damage can be caused to the profession and to industry by ill-informed lesser officials relying blindly upon a Code without the knowledge to apply it with discretion—

> " But man, proud man,
> Drest in a little brief authority,
> Most ignorant of what he's most assur'd,
> His glassy essence, like an angry ape,
> Plays such fantastic tricks before high heaven
> As make the angels weep."

After 1945, the ability to move about the world once more led to an extensive interchange of ideas. Government missions to Germany in 1945 proved that there had been little advance in that country during the war compared with our own, except for L. Casagrande's use of electro-osmosis in difficult silts. So far as Great Britain is concerned, vigorous contacts and co-operation between European workers and the U.S.A. culminated in the Second International Conference on Soil Mechanics at The Hague in 1948, held concurrently with the Second Conference on Underground Town Planning.

This Conference led to the formation of the International Society of

Soil Mechanics and Foundation Engineering, to keep alive the general interest by forming National Committees, and preparing for the Third Conference in 1953.

The British National Committee has six members nominated by the Institution and six elected by the subscribing members of the British Section, which, in 1952, number 129. Although this Section is independent of the Institution, the secretarial work and use of rooms for meetings is provided by our Institution. The British Committee reports each year to the International Society. The report is divided into twelve sections, and members are invited to give a short reference to the work which they have carried out during the year in any section, so that those interested can communicate with them and exchange experiences.

In 1950, the British Section organized a European Conference on the Shear Strength of Soils, which was held at the Institution.

The result of these international exchanges and visits to European countries led to personal friendships and correspondence between workers in these fields. A small group of English workers formed the Geotechnical Society and founded a private journal called "*Géotechnique*," published in French and English, being supported by subscribers in twenty-eight countries. This magazine appeared twice yearly and was devoted to Articles and Papers on Soil Mechanics and related subjects. After two years of successful and increasing circulation the free labour and time of the enthusiasts who started it was becoming overburdened, and in 1950 the Institution of Civil Engineers took over the secretarial work, publication, and distribution, and nominated an editorial advisory committee. This healthy child of enthusiasm has now become the organ of the British Section of the International Society, and is issued to subscribing members. The Proceedings of the Shear Strength Conference were published in the numbers for December 1950 and June 1951.

At the Second Conference it was emphasized that the practical application of Soil Mechanics requires a compromise between the methods of the exact sciences such as the theory of structures, and those of the empirical ones, like geology. This truth is not always welcome to the ultra-mathematical mind. One Dutch engineer quoted an engineering physicist who had said to him "I do not see the difficulties in soil mechanics. You have only to draw up the appropriate differential equation for every problem and solve it to be able to calculate all you want."

Recent developments have shown the relation between the actual execution of works and the soil mechanics problems of foundation stability. Terzaghi in his Lecture emphasized that difficulties with soils arise almost exclusively, not from soils themselves, but from the water contained in their voids—a remark which almost echoes Gregory in 1844. This applies not only to their strength or loss of strength, but also to difficulties met with in actual construction work.

In one of the four Institution Lectures on Soil Mechanics in 1946 the distinction was made between methods by which the load is suited to the soil, and methods of suiting the soil to the loads. These covered methods of compaction, both superficial and in depth, artificial cementing, removal of water by various methods, " exclusion " of water by cofferdams, and such refinements as base exchange and electro-osmosis.

The most important tests affecting this branch are sizing analysis, permeability, and density determinations. The title " Geotechnical Processes " was applied to these methods and their relation to Soil Mechanics as a science was established in those Lectures, and is a British contribution, as were many of the original steps in the field, even if—as so often happens—others carried out the interim exploration.

Freezing was undertaken in mine shafts for the first time in 1862 in South Wales as a means of overcoming water problems. The principle of pumping to draw water away from works by means of a number of wells instead of to them by sumps, which was first applied by Stevenson at Kilsby Tunnel, was later developed as groundwater lowering with filter wells in Germany, with remarkable results, by using deep-well submersible pumps, especially in artesian waters. At the same time a simplified form of well-point pumping developed in the U.S.A. was originally based on the British driven pipe-tube well, which had been used by the British Forces in their successful advance to Magdala in Abyssinia in 1868.

Following the use of cement and lime grouting behind Greathead's tunnel linings, cement injections were developed. The injection of chemical solutions was found to be a means of cementing sands. Bitumen, clay, and asphaltic injections followed.

In a Paper [19] on " The Choice of Expedients in Civil Engineering Construction " the theme was developed and examples given, urging the necessity for sizing curves to be published, when quoting examples for records, in order to show not only what was done, but also the type of soil in which it was done. The choice of expedients is closely related to the grain size of the soils concerned, as well as to the means of access to the particular soil which requires treatment. It has been a considerable scientific advance to be able to recognize the limits in which success may be possible, and not to be carried away by hope that success can be obtained in unlikely conditions.

The neglected factor of time is also important in drainage. Many works have been made more difficult than necessary by ignorance of the laws of the flow of groundwater, and of the fact that slow drainage can achieve success in cases where undue impatience could cause failure. Engineers and builders sometimes boast of works where they have " overcome a difficult pumping problem," when in reality their difficulties have been largely of their own making.

Electro-osmosis has been written up to some extent following successes in the war at Trondheim in making excavation possible in a fine silt.

Such a process is dependent upon the soil being suitable, and it can be obstructed by varied soil conditions in the same way that injections or drainage can be obstructed by impermeable or unsuitable beds. In Great Britain thick beds of silt seldom occur, and so this process has had less suitable fields for application.

In the first James Forrest Lecture the interdependence of abstract science and engineering was insisted upon by Dr Anderson, and made the theme of these Lectures. It is stressed by a warning which he gave, and which Terzaghi quoted : " There is a tendency among the young and inexperienced to put blind faith in formulae, forgetting that most of them are based upon premises which are not accurately reproduced in practice, and which in any case are frequently unable to take into account collateral disturbances which only observation and experience can foresee and common sense provide against." Terzaghi's reason for quoting that sentence was that in 1939 he was greatly concerned about the self-confidence inspired in many members of the incoming generation of engineers by new knowledge unaccompanied by experience. This concern about over-confidence is well justified. During the development of the new science some young men were better acquainted with recent theories than older and more experienced men, whose very eminence and immersion in their work handicapped them from studying in detail the more recent writings. No amount of theory can succeed unless balanced by some practical experience in actual execution of work in order to understand not only the varieties of the ground, but also the limitations, practical and economic, within which it is possible for man to act.

The saying of Dr Hamilton on Perronet is worth quoting once more in relation to Dr Terzaghi himself :

" His work, like that of Smeaton, Telford and Rennie in this country, was broadly based on shrewd intuition, backed by a capacity for applying theory when he found it helpful, without being intimidated by it when it conflicted with his judgment."

Conclusions

In this Lecture an attempt has been made to leave behind an account of how the new chapter in engineering science has expanded to the conclusion of its first volume. Those who wish to study it in detail should consult the seven volumes reporting the Second Conference, the four Lectures published by the Institution, and the articles in " *Géotechnique.*" Soil Mechanics is also dealt with in the Codes of Practice for Foundations, Earthworks, and Earth Retaining Structures.

British soil scientists, though slow to start compared with transatlantic developments, have since expanded rapidly, until the British school of soil mechanics thought and experience is as developed as that of any country.

I have attempted to show how throughout it has been the influence and encouragement of this Institution which has helped the research and recorded in Papers and Lectures the results obtained, and, by organization and hospitality to international societies, helped to foster the interchange of ideas and experience.

When young engineers first read our Proceedings they may feel that they draw little benefit from them. It is only after amassing some experience that the searcher can see the real wealth of information and value which they contain. The small points in odd Papers add up to a considerable body of fact. They are the work of many minds, and contributions to a discussion, by recording or confirming an experience, can add to the vast accumulation of knowledge which can only be glimpsed by those with the time, interest, and experience to delve into the older Papers of predecessors.

Let us hope that many more will be presented, for it is only by the combined efforts of all its members that our great profession can expand its knowledge to the use and convenience of man.

In Boswell's London Journal is a saying of Dr Johnson which will serve as my conclusion :

" He said that human experience, which was constantly contradicting theory, was the great test of truth. Experience built upon the discoveries of a great many minds was always of greater weight than the mere working of one mind, which can do little of itself."

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Mr W. K. Wallace, who proposed a vote of thanks to Mr Harding for his Lecture, said that to some extent he was wasting time in doing so, because it was quite obvious to everybody that the meeting had enjoyed the Lecture and, in enjoying it, had recognized its value. Soil Mechanics was one of the children which had grown up very rapidly and, if it had not come of age, at least it had attained a very considerable stature. Mr Wallace's first contact with Soil Mechanics, although it was not then given that name, had been through reading Professor Terzhagi's earlier articles in the technical press. Subsequently, he came into more direct contact with it when the Building Research Station asked railway engineers to give their staff opportunitites of seeing any interesting cases of failure.

Mr Wallace remembered a number of gentlemen arriving from Garston to look at retaining walls which had ceased to retain and at structures which were intended to be static but were, in fact, moving; and he remembered having some trouble with some of the more senior members of the staff who were rather hurt by the idea. Those senior members had been running a district for about 25 years and then had had this unfortunate failure, whatever the cause of it might have been; and then a youngster had come down from the Building Research Station—a youngster whom they would scarcely have allowed to hold a tape when measurements were being taken—to tell them how it had happened.

Mr Wallace said he had felt some sympathy with those senior men, because there was a curious delight in a failure among the visitors from the Research Station. He recalled going to see a wall which had been giving some trouble—it had been moving and producing a most peculiar gradient on a road which was supposed to be flat—and he had found a Building Research Station man, with two assistants, boring holes in the ground. He had asked how they were getting on, and the reply was, "Splendidly, it is moving so fast that when I take it out of the hole,

clear it, and then try to put it down again, it has moved far enough in the interval to prevent me from getting it down the old hole. I know exactly the plane of cleavage." Whilst a maintenance engineer would not be expected to behave as if he were at a funeral on such an occasion, he should at least express regret; instead of which that young man was absolutely delighted at an incident which was upsetting the lives of a lot of people. It was not unreasonable, therefore, Mr Wallace commented, that such visitors had not been very welcome.

But Soil Mechanics had lived that down, and that was partly because people had grown older and more tactful. It was also partly because they had become more accustomed to it. It should be remembered that very often, as an engineer advanced in his profession he became less of an engineer and more of an administrator, and it was therefore difficult for him to keep in touch with the latest ideas and methods. When such a man, after years of practical experience, was suddenly and rather abruptly corrected by somebody very much his junior, the reaction was apt to be, if not physically violent, at any rate mentally violent.

It was well that Soil Mechanics had developed to such an extent. It was now part of the regular procedure to insist upon a proper investigation being made before undertaking a job.

Mr Wallace said that Mr Harding had given a masterly analysis on how the advance had proceeded during the last decade and members were indebted to him for giving such an excellent résumé.

Dr Guthlac Wilson, who seconded the motion, said that the members had listened to a very fascinating double-duty lecture by Mr Harding, the first part of it being historical and the second part an appreciation of the present position. Both parts were very valuable. It had been extremely interesting to realize how far things had advanced in the period since the beginning of the war.

Dr Wilson said he wanted to add one small personal recollection. It was of something which happened to him in 1939, but which he thought could not happen today. He had just returned after spending a year in America studying the subject and he had been asked, " What have you been doing ? " When he explained, the comment was, " That will not be of any value to you over here. We have such excellent soils in England."

The vote of thanks was carried unanimously, with acclamation.

Paper No. 5859

"The Measurement of Very Slow Movements in Large Structures" *

by

Harold Leslie Cox and Stanley Edwin Mitchell

(*Ordered by the Council to be published with written discussion*) †

SYNOPSIS

A method is described for the detection and measurement of movements of structures which seem to be distinct from original settlements and continue slowly over a long period of time. Special micrometers are used to indicate the changing relative positions of two steel pegs to which the movement is communicated. The readings are accurate under favourable conditions to 0·0001 or 0·0002 inch.

The application of the method for some years at the Tower of London and Rochester Bridge has revealed what appear to be persistent movements of whole structures through a few hundredths of an inch each year. Comparative measurements at one position in the Tower of London suggest, further, that one stratum is moving with respect to another below the surface of the earth.

Previous observations by other workers, who found movements of the same order of magnitude in London and elsewhere, are briefly mentioned.

Part I.—Origin and Development

Introduction

Any compact mass left standing on the surface of an imperfectly elastic medium may move in relation to the body of the medium as the surface material deforms. When the supporting layers first take the weight of the imposed mass the settlement may be quite rapid, and in designing foundations for large structures the effect of this settlement must always be anticipated; in particular, damage caused by excessive or irregular settlement must be prevented.

This Paper is not concerned with the comparatively large and rapid movements of original settlements, but discusses the very much slighter movements which may continue slowly for years—or even centuries—and are therefore termed "secular." Such slow movements may be unimportant, but from time to time their presence is revealed by cracks in masonry

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† Correspondence on this Paper should be received at the Institution by the 1st March, 1953, and will be published in Part I of the Proceedings. Contributions should be limited to about 1,200 words.—Sec. I.C.E.

buildings or by other signs of slight relative movement of the several parts of a large structure. These signs have, sometimes, prompted investigation of the movement to decide whether or not remedial treatment is necessary.

In the past, settlement has often been measured with the aid of surveyors' levels. A different method was used in observing settlement of the piers and abutments of the new Waterloo Bridge [1]; a tube, passing down a borehole, was fixed to a shoe driven into the clay and moved inside another tube fixed to the foundation of the pier or abutment. The relative vertical movement of the tubes indicated the settlement and was read from a dial gauge at the top. Another instrument, described by the Post Office Engineering Department, measured the change in height of one structure in relation to another by making use of the common water-level in two cups connected by a long flexible tube.[2, 3, 4, 5]

The method of investigation about to be described has been developed by the staff of the National Physical Laboratory on the basis of special micrometers originally devised by Sir Horace Darwin, F.R.S., for making observations on cracks in the masonry of St Paul's Cathedral. At first, Darwin's method was applied directly to record the movement across cracks in the structure of the Tower of London. Later, however, it was extended to measure the movement of whole parts of the buildings in relation to separate reference piers specially erected. Later still, a further extension made possible the recording of very small movements between widely separated parts of a large bridge. Throughout the work, but more particularly in the later developments, factors other than time, such as temperature and tidal movements, have had to be allowed for.

Meanwhile, for the past 25 years, similar micrometers have been in constant use at St Paul's Cathedral for the measurement of relative movements across cracks in the masonry. By the courtesy of the Consulting Engineers, Messrs Freeman, Fox and Partners, and of the Surveyor to the Fabric, Mr Godfrey Allen, the Authors have been afforded access to these records. The measurements at St Paul's form one part of thorough surveys which are made at regular intervals, and a great quantity of data has been compiled. The measurements made with the micrometers, in general, describe the actual relative movement across cracks in the masonry or of one part of the structure relative to another; but the measurements as a whole are related to the underlying subsoil gauged with regard to a deep-level datum point in a shaft in the yard of the King Edward building of the General Post Office. Several of the measurements across cracks in the masonry of St Paul's show more regular and progressive movements than those recorded at the Tower, but the general trends of the two series of observations are otherwise similar, both in type and in magnitude.

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[1] The references are given on p. 706.

Movement at Cracks at the Tower of London

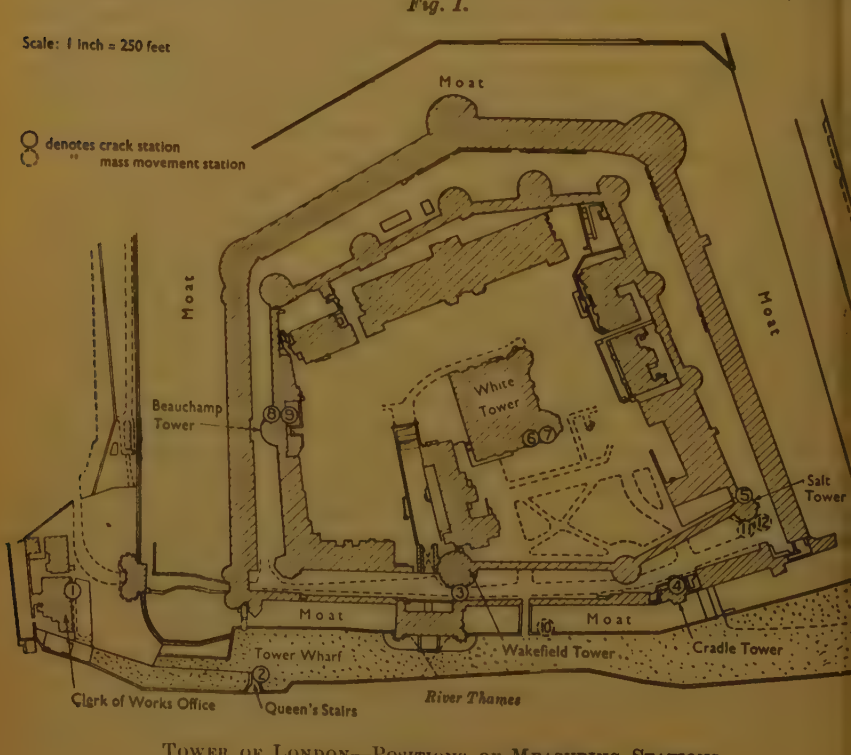
In December 1914, at the request of the First Commissioner of Works, Dr (later, Sir Thomas) Stanton, Superintendent of the Engineering Department of the National Physical Laboratory, inspected various cracks in the buildings of the Tower of London; his task was to consider how their rate of growth might be measured and to estimate the possible danger to the buildings. In 1915, he began periodical observations of selected cracks.

Micrometers of Sir Horace Darwin's design had been constructed by the National Physical Laboratory, using accurate micrometer screws supplied by the Cambridge Scientific Instrument Company, Ltd. The micrometers indicate very precisely the relative displacement of the two sides of a crack as transmitted to steel pegs set in the masonry; their use is described more fully in Part 2.

A plan of the Tower of London (*Fig. 1*) shows the positions of the nine measuring stations at which cracks were observed. Some are in outdoor and others in indoor situations.

Several years' observation showed that the cracks were not threatening

Fig. 1.



Tower of London—Positions of Measuring Stations

the stability of the buildings. It seemed possible that any progressive shift of the foundations had resulted not from unequal settlement but rather from movement in mass of the subsoil on which the foundations rested. The investigation of this possibility led to an important extension of the method of measurement.

Movement in Mass at the Tower of London

The Quay Wall

Some signs of a mass movement had been observed in the Quay Wall of the Tower. Fortunately the Moat on the land side of the Quay Wall offered a very convenient situation for an attempt to measure this movement. Close to the Quay Wall a vertical shaft was sunk through the ballast layer on which the Wall itself was founded and down to the level of the blue clay, which at this point was only about 7 feet below the surface of the ground in the Moat. A brick pier was built up from the blue clay in the clear space provided by the shaft, and steel pegs similar to those used at the cracks were fitted to the top of the pier and to a block projecting from the adjacent wall (*Fig. 2*). The special micrometers here recorded the movement in mass of the Quay Wall in relation to the blue-clay stratum below.

The observations, begun in 1919, soon demonstrated that mass movement was indeed taking place, and after a few years the magnitude and direction were established. The Quay Wall was found to be moving downwards and away from the river, in a direction at about 45 degrees to the horizontal and at a rate of a few hundredths of an inch each year. Superposed on this secular movement there were seasonal movements (having a yearly cycle) and tidal movements (having a twice-daily cycle) of considerable magnitude.

The Salt Tower

The cyclic movement of the Quay Wall in response to tidal variation made it necessary to consider whether or not the slow secular movement might also be an indirect result of the rise and fall of the tide and, in order to obtain further data, two additional measuring stations were set up on the south side of the Salt Tower, where the direct effect of the tide would be absent. Two piers similar to the one already in use in the Moat were constructed, one built up from the blue clay, starting about 25 feet below the surface, but the other only from the ballast above the blue clay. Measurements with the special micrometers were begun in 1926 and indicated the movement of the masonry of the Tower in relation to the blue clay and to the ballast respectively. By combining these results the movement of the ballast in relation to the blue clay was also indicated.

The movements of the Salt Tower have been of the same order of magnitude as those of the Quay Wall. (A fuller description is given in Part 3.)

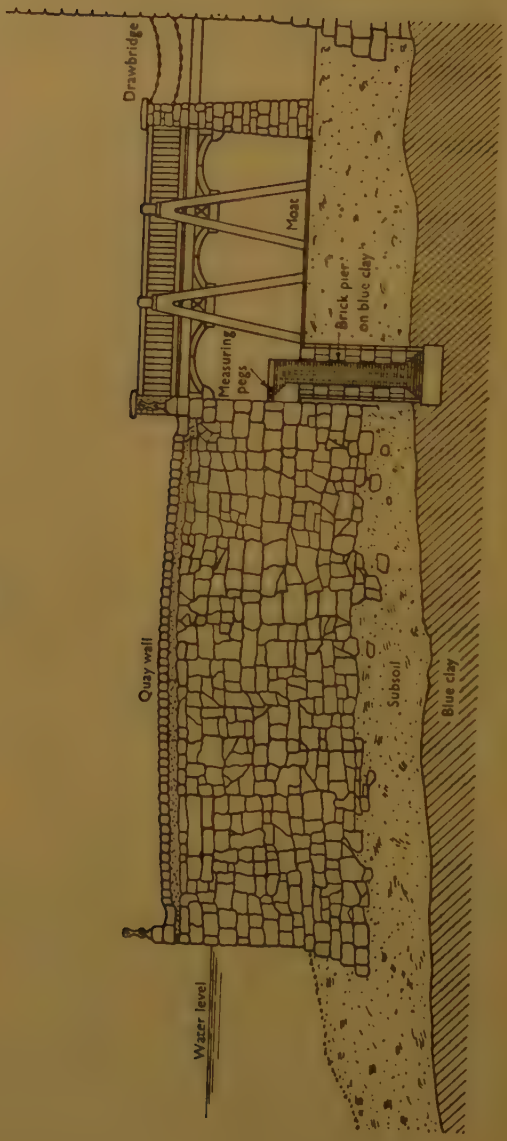


Fig. 2.

MEASURING ARRANGEMENTS AT QUAY WALL

Movement at Rochester Bridge

In 1942, there arose an opportunity to apply the same general method of measurement in a rather different way. The consulting engineers to the Rochester Bridge Trust approached the National Physical Laboratory with reference to a slow but apparently steady movement of part of the structure of Rochester Bridge, which was progressively reducing the gap provided in one of the expansion joints. The damage to the superstructure was not sufficient to suggest any urgent need to consider the strengthening of the foundations; but after consultation with the Building Research Station it was decided, as an initial measure, to apply a simplified form of the method used at the Tower of London, in the hope of affording early warning of any serious acceleration of the movement.

The observations undertaken with this limited objective have proved to be far more informative than was originally expected. In addition to confirming that there is no immediate danger to the structure, they have recorded a fairly steady and progressive movement, apparently of the Strood Abutment, of a few hundredths of an inch each year toward the centre of the stream. The precise mechanism of the movement is not yet well understood—partly on account of the indirect method employed, of which details are given in Parts 2 and 3. The observations are being continued, and it is hoped that it will soon be possible to supplement them by periodical measurements with a very sensitive plumb-line apparatus to determine whether or not the movement is strictly a translation or wholly or partly the result of tilting of the abutment. This should also afford evidence to confirm the fairly clear indication from the present measurements that the movement is of the Strood Abutment as a whole. Moreover, the pattern of the tilting movement in relation to time should be of great value in determining the reality of an apparent seasonal variation in the rate of movement indicated by the present measurements.

Part 2.—Apparatus and Technical Procedure

Plaster Tell-Tale Method

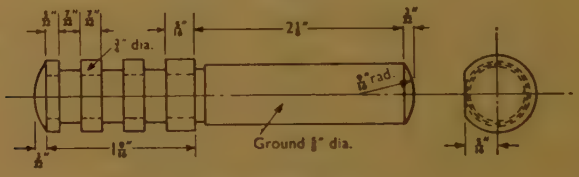
A very simple method often employed to observe movement at a crack is to fit a plaster tell-tale across it. The tell-tale consists of a block of cement, about 3 inches by $1\frac{1}{2}$ inch, keyed into the masonry (not merely applied to the surface) at each side of the crack. Movement is shown by the appearance of a crack in the cement, and if the movement is considerable, periodical measurement with a rule will provide a rough record of its progress.

The micrometer method described in this Paper enables both the magnitude and the direction of the relative movement to be determined with much greater accuracy than is possible with the plaster tell-tale.

Fitting of Pegs at the Tower of London

A drawing of one of the hardened and ground steel measuring pegs use at the Tower of London is given in *Figs 3*. A hole a little larger than th ridged part of the stem is cut in the masonry on each side of the crack und

Figs 3



Measuring Peg

investigation, and a peg is cemented in each hole. A special clamp secur the pegs until the cement has set. Between measurements the pegs a protected by greased caps.

Tower of London Micrometers

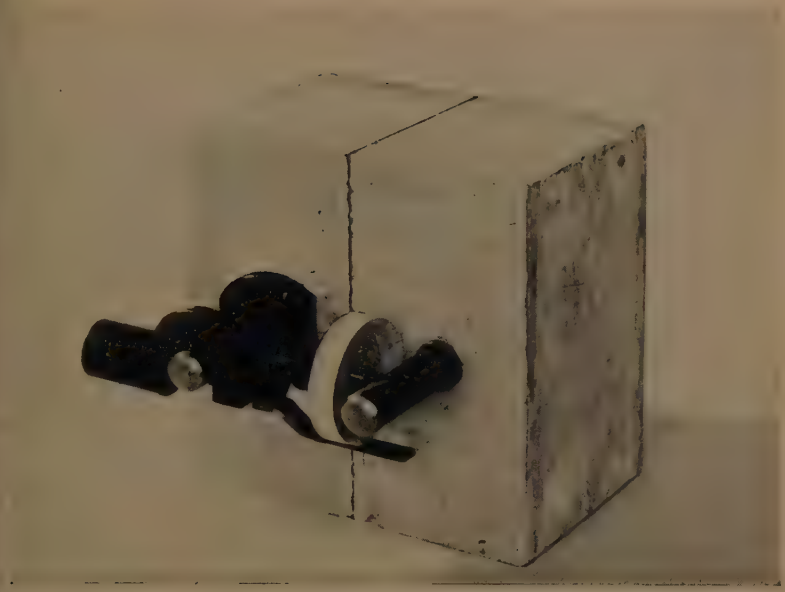
Figs 4, *5*, and *6* show the micrometers used at the Tower of Londo each in position on a replica of the measuring pegs. These micrometer measure relative displacement of the pegs in three mutually perpendicul directions.

Micrometer A, illustrated in *Fig. 4*, measures lateral movement, that to say, displacement perpendicular to the axes of the pegs in the plan containing these axes. This micrometer has a saddle with V-groove which afford a positive location on one of the pegs, and the rear face of th saddle is brought into contact with the shoulder provided at the base the peg. The saddle carries a micrometer screw with two threads per mill metre, and the large white head rotating past a brass scale projecting fro the bottom of the saddle is graduated into 100 divisions.

A balance weight is fitted to the micrometer at the side remote fro the micrometer head, and with this balance, by rotating the saddle throug a small angle on the peg on which it rests, a very delicate indication contact is obtained as the rounded end of the micrometer brushes the sid of the free peg. With care in observation, the sensitivity is reliable to plu or minus a half of one division on the head ($\pm$ 0·0025 mm.). It is standa practice to take the average of readings obtained with the micromet saddle resting on the left-hand and right-hand peg in turn.

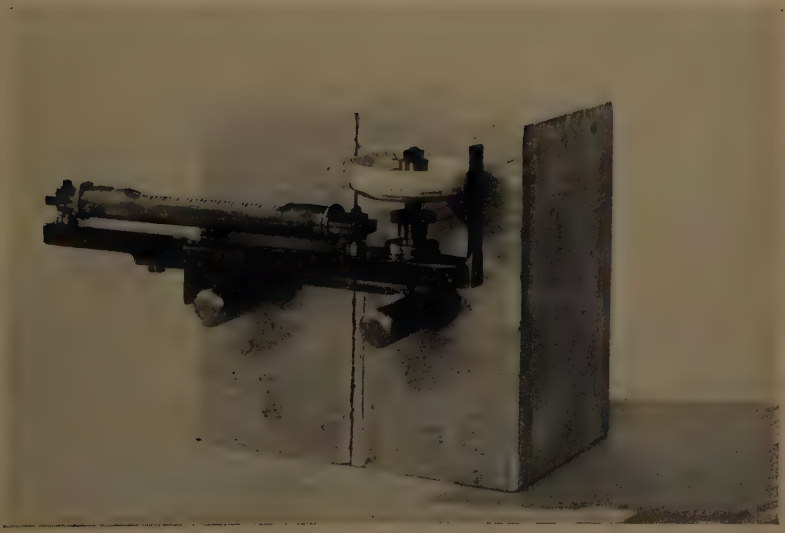
Micrometer B, illustrated in *Fig. 5*, measures the relative vertic displacement of the pegs. Like micrometer A, it is provided with a saddl but the top of the saddle is extended on each side to carry a spirit-level the micrometer screw is vertical and has a flexible steel blade interpose

Fig. 4



MICROMETER A

Fig. 5



MICROMETER B

Fig. 6

MICROMETER C

Fig. 9

CALIBRATING DEVICE

etween its tip and the measuring peg. The reading is taken with the pirit-level horizontal.

Micrometer C, illustrated in *Fig. 6*, measures the relative displacement f the pegs in the direction of their own axes. The locating saddle of this nicrometer is developed at the top into two curved arms, each of which as a flat ground on to its end. Two balance weights give stability. The nicrometer screw passes through a block carried in front of the saddle, nd bears on the rounded end of the measuring peg. The reading is taken when the ground flat on the curved arm just brushes the rounded end of he free peg.

When movement carries the separation of the measuring pegs beyond he range of any of the micrometers, a sleeve or extension can be fitted o one of the pegs while an observation is being made.

Correction of Readings from the Tower of London

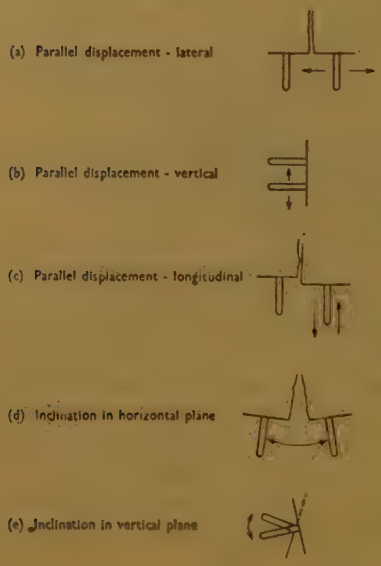
In the case of micrometers B and C also it is standard practice to take eadings with the saddle resting on the left-hand and right-hand pegs in urn, but the separation of the pegs in this case is represented by the difference between the two readings. Conversely the sum of the two readngs is independent of the movement of one peg relative to the other and herefore should remain substantially constant. Inspection of the sum of he readings thus provides a check on the individual readings, whilst he absolute magnitude of the sum also indicates whenever a sleeve was n use.

Apart from these overall checks on the possibility of gross misreading, he small variations of the sum from a truly constant value depend both on the absolute sensitivity of the measurements and on the secondary ffects of one type of displacement on the measurement of another, and of elative rotations of the pegs.

The five possible types of relative movement are indicated diagrammatially in *Figs 7* and it will be seen that movement of type (*a*), for instance, vill affect the reading of micrometer B, because the point of contact of lexible blade with the peg will be displaced from the point of contact of he micrometer-screw tip with the blade (see *Figs 8*). The several secondry effects of this kind could be estimated theoretically from the geometry f the micrometers and pegs; but the most important of them are more eadily determined experimentally by use of the calibrating device illusrated in *Fig. 9*. This consists of a 6-inch length of 4-inch-by-3-inch teel angle-bar, with the bottom face ground true and the two vertical aces ground parallel to each other and perpendicular to the bottom face. Two slots, each $1\frac{1}{4}$ inch long and at right angles to each other, were nachined in the vertical part of the angle, and two pegs, similar in the ssential features to the measuring pegs at the Tower, were bolted through he slots and could be moved to simulate the relative displacement of the

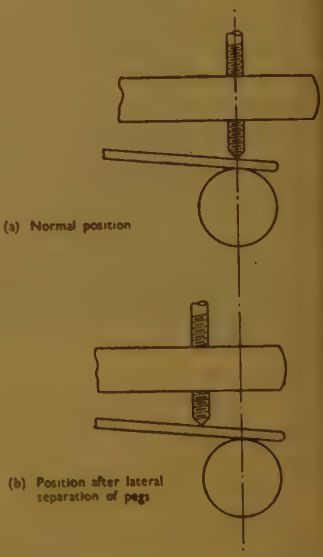
measuring pegs. Readings in a number of different positions were take with micrometer A and micrometer B, and it was then possible to state th correction necessary to the reading of micrometer B for any measure

Figs 7


(a) Parallel displacement - lateral
(b) Parallel displacement - vertical
(c) Parallel displacement - longitudinal
(d) Inclination in horizontal plane
(e) Inclination in vertical plane

POSSIBLE KINDS OF RELATIVE MOVEMENT OF PEGS

Figs 8



CONTACT CONDITIONS FOR MICROMETER B

peg-position. The necessary modification to the nominally consta sum of corresponding readings was also given by these results.

SITE CONDITIONS AT ROCHESTER BRIDGE

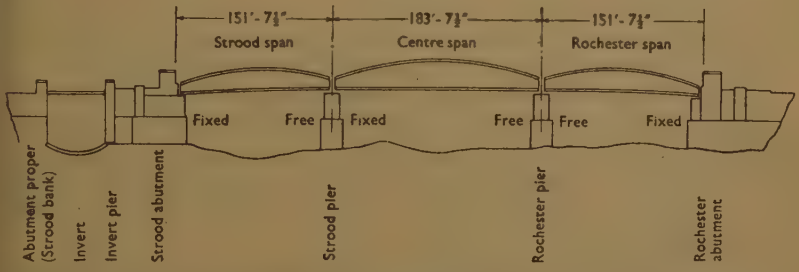
At Rochester Bridge the requirements are rather different from tho at the Tower of London. Horizontal movement only has been measure and therefore only one micrometer, similar to micrometer A (*Fig. 4*), h been employed. The screw has twenty threads per inch and the head graduated in 100 divisions. An extension-cap screwed on to the micr meter stock during the winter months enables the instrument to accommoda the large seasonal movement.

The elevation of the bridge is shown diagrammatically in *Fig. 10*, a in this diagram the designation " free " has been used to denote those en of the three separate spans which are supported on rollers and are th capable of longitudinal movement over the piers. In addition to the thr main spans, which are of open-braced construction, there is a small sectio

at the Strood side which spans an inverted arch not always submerged at low water.

The bridge was completed at some time between the years 1914 and 1918; the superstructure was supported on the piers and abutments of the previous cast-iron bridge, which itself had been completed in 1856.*

Fig. 10



Diagrammatic Representation of Rochester Bridge, looking Downstream (North East)

These are supported on cast-iron cylindrical piles filled with concrete and brickwork after excavation under air pressure.[6]

Method Used at Rochester Bridge

When, in 1942, the present series of measurements was undertaken, the main object was to render quantitative the previous qualitative observations on the closing of the expansion joint; but at the same time the opportunity was taken to try to trace the course of its progressive movement.

The only feasible method was, in effect, to measure across the joint itself, although for convenience the measuring pegs, in the free end of the Strood span and in the fixed end of the centre span (see *Fig. 10*), were fitted to the webs of the main girders below the decking of the bridge. The measurements thus recorded would of course include the large movements due to thermal expansion of the Strood span, and they might also include movement caused by tidal pressure like that observed at the Quay Wall of the Tower of London. Various methods of eliminating these superposed movements were considered, but none appeared to offer the requisite degree of accuracy without elaborate and impracticable constructional work. However, if only a first order correction for the effect of temperature and tide were sufficient, some help could be gained from the fact

* The predecessor of the 1856 bridge, a stone bridge built in 1387, had stood a little further upstream, but two earlier bridges—one erected on timber foundations about the year 950, and the other a Roman bridge—had crossed the river where the present bridge stands.

that the Rochester span was equal in dimensions to the Strood span; it seemed that the effect of temperature and tide should be at least approximately the same on both spans. It was, therefore, decided to take comparative measurements on the Rochester span.

Two pairs of pegs were used at each pier, one pair being fixed near to each side (upstream and downstream) of the roadway so that a mean value could be taken.

To determine the general pattern of the progressive movement and the effect of temperature and tide, it was clearly desirable, at least at the start of the investigation, to take frequent observations. For reasons of economy and convenience, therefore, the readings were taken by the Bridge Caretaker, and his use of the special micrometer was supervised and checked by members of the National Physical Laboratory staff, who made periodical visits to the bridge for that purpose. For some months readings were taken daily; later the frequency was reduced, and recent readings have usually been taken once a week.

In the hands of an observer not previously experienced in this particular type of work the special micrometer proved its merits. So long as the saddle is fitted squarely on the peg and in contact with the shoulder provided at the base of the peg, the change from the "go" to the "not go" condition can easily be felt when the micrometer head is turned through one division on the scale ($\frac{1}{100}$ of a complete revolution, or 0·0005 inch). The new observer was very soon proficient in recording to this degree of sensitivity. In practice the effective sensitivity is governed less by the inherent sensitivity of the measuring device than by transient movements, occasionally as great as 0·003 inch, resulting from the passage of heavy traffic over the bridge.

The situation of the observer under the roadway of the bridge is far from enviable, and the scales, particularly the brass scale of units, are not always easy to read. The few accidental errors were nearly all in the units place and were easily traced and corrected by comparison of the readings on the upstream and downstream sides of the bridge.

Part 3.—Results

Chief interest lies in the movement in mass. This type of movement, at both the Tower of London and Rochester Bridge, has exhibited a regular pattern, though it has varied between the different sites.

Movements at Cracks

By comparison with the records of movement in mass, those obtained from measurements across actual cracks are very disappointing. A summary of the latter is given in Table 1. In only two cases has any

TABLE 1.—SUMMARY OF MOVEMENTS OF CRACKS AT THE TOWER OF LONDON

(Pegs approximately north and south of each other except at Station 7)

Measuring station		Period	Movement of northern peg with respect to southern peg : millimetres		
Identification number	Description		Lateral	Vertical	Longitudinal
1	Wall of Clerk of Works' Office (station dismantled 1934)	1915 to 1934†	0·31 (opening)	0·05 downward	0·07 forward*
2	Queen's Stairs	1915 to 1948	10·2 (opening)	4·1 upward	3·6 backward*
3	Arch adjoining Wakefield Tower	1915 to 1946	Inappreciable	Inappreciable	Inappreciable
4	Cradle Tower	1915 to 1930†	Inappreciable	Inappreciable	Inappreciable
5	Window in Salt Tower	1915 to 1948	8·0 (opening)	0·2 downward	1·3 forward*
6	Pier in triforium of St John's Chapel	1915 to 1948	0·2 (opening)	Inappreciable	Inappreciable
7	Arch in triforium of St John's Chapel (adjoining Station 6; pegs approximately E. and W. of each other)	1915 to 1948	Inappreciable	Inappreciable	Inappreciable
8	Arch over doorway in Beauchamp Tower	1915 to 1948	1·7 (opening)	0·4 upward	Irregular over a total range of 0·4
9	Arch over window in Beauchamp Tower	1915 to 1948	Inappreciable	Inappreciable	Inappreciable

* "Forward" signifies protrusion, and "backward," retraction of the northern peg with respect to the southern

† Measurements discontinued

significant movement been recorded, and in neither of these has any clear pattern of movement in time been observable. Station 2 is close to the foreshore, and the comparatively large movement recorded there may be due to the shifting of only a small part of the masonry at the edge of the structure. At Station 5 the opening of the crack above the window in the Salt Tower has not been traceable to any simple movement of a part of the Tower.

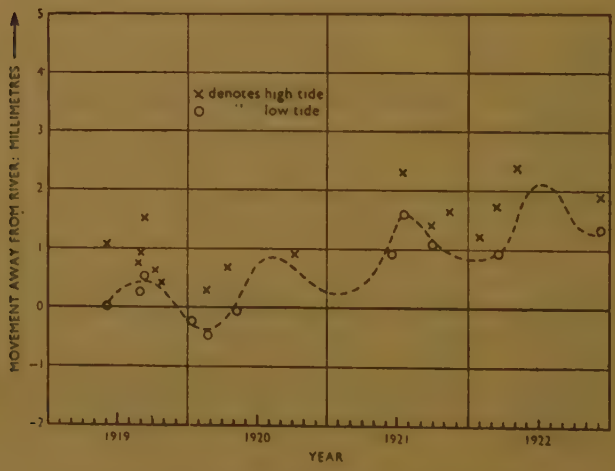
Movement in Mass—Quay Wall

Effect of Tide and Temperature

In order to abstract a record of secular movement, whether it be movement at a crack or movement of the whole body of a structure, the immediate effects of tide (if appreciable) and temperature must first be eliminated.

At the Quay Wall the varying pressure resulting from the rise and fall of the tide has a substantial effect on the lateral displacement (micrometer A). The curve for lateral displacement over a short period of time is shown on an enlarged scale in *Fig. 11.* The deviation of the readings taken at high tide from those taken at low tide is evident. The broken-line curve, which represents the measured displacement values, is drawn always through the "low tide" readings to preserve consistency. The wavy form is caused by the effect of temperature, which causes expansion of the Wall in summer and contraction in winter.*

Fig. 11



Tide Effect on Lateral Movement of Quay Wall

The vertical displacement (micrometer B) and longitudinal displacement (micrometer C) are very little affected by the rise and fall of the tide. They are affected by temperature changes, but to a smaller extent than the lateral displacement.

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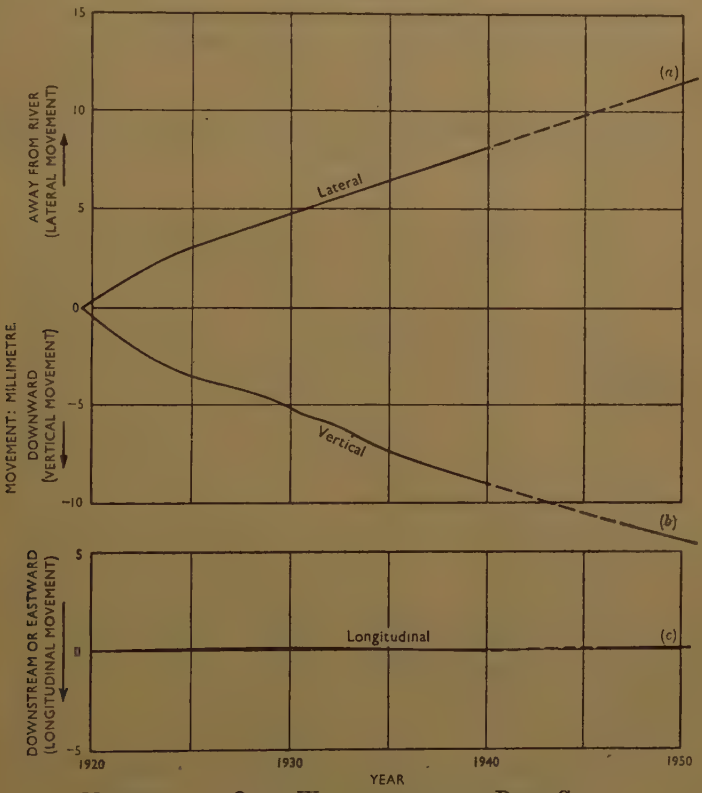
* This wavy form is found also, of course, in curves of movement at cracks. It is understood that the same phenomenon, of a yearly cycle to and fro superposed on the true secular movement, appears in the measurements on cracks at St Paul's Cathedral.

Magnitude of Secular Movement

By drawing a mean line through the wavy curve the seasonal variation is eliminated and the course of the secular movement is revealed. Thus in *Figs 12*, curve (*a*) shows the lateral movement, in the direction of the width of the Wall (which is approximately north and south); curve (*b*) shows the vertical movement; and curve (*c*) shows the longitudinal movement, in the direction of the length of the Wall (which is approximately east and west).

Figs 12



Movement of Quay Wall relative to Blue Clay

During the 31 years over which observations have been taken, the Wall has shifted longitudinally no more than 0·1 millimetre eastwards in relation to the blue clay.

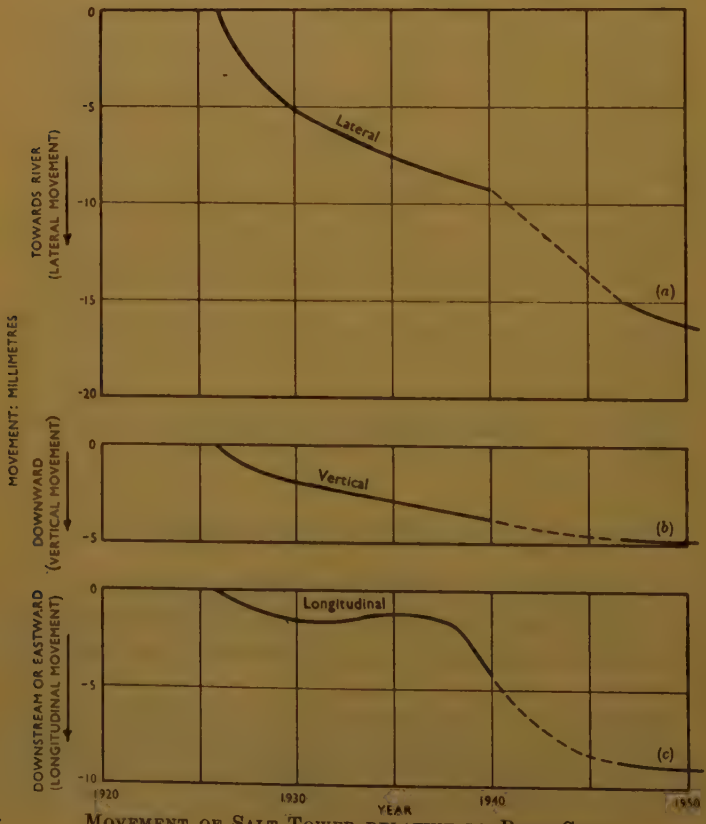
The lateral and vertical movements have, however, been much greater. Over any given period of time, the lateral movement (northwards, or away from the river) has been nearly equal in magnitude to the vertical movement (downwards), and the total shift in each direction during 31 years has been about 12 millimetres.

Though the brick pier which carries the peg forming the practical reference datum is no doubt itself subject to some initial settlement, the pier is an extremely small structure in comparison with the Quay Wall, and its own settlement is unlikely seriously to mask the movement of the Wall with respect to the blue clay. The trend of the curve for vertical movement (*b*) in *Figs 12*, supports this view. The initial settlement of the pier would be expected to decrease rapidly with time, and the effect of such settlement on the curve would be to make a uniform downward movement of the Quay Wall with respect to the clay appear to become more rapid with the passage of time. Actually the curve shows the reverse trend, and therefore the settlement of the pier is unlikely to have had a great effect.

Movement in Mass—Salt Tower (and Comparison with Quay Wall)

At the Salt Tower, owing to its distance from the river, no allowance has to be made for the rise and fall of the tide. Seasonal variations are

Figs 13

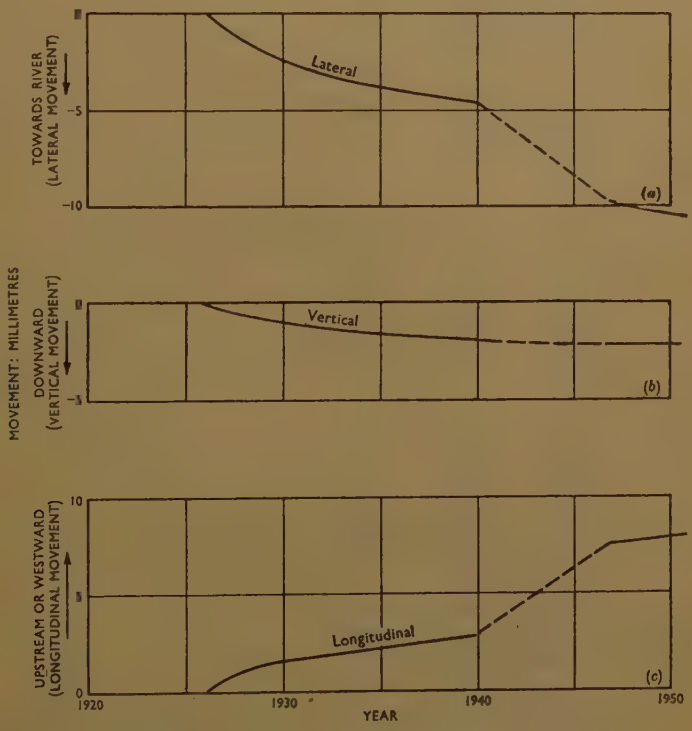


Movement of Salt Tower relative to Blue Clay

eliminated by drawing a mean line, as for the measurements at the Quay Wall.

Figs 13 show the secular movements indicated for the Salt Tower relative to the blue clay: (*a*) the lateral movement; (*b*) the vertical movement; and (*c*) the longitudinal movement. It is clear that, although the Salt Tower, like the Quay Wall, is sinking with respect to the blue clay, its horizontal movement is towards the river. The pattern of the longitudinal movement, however, is rather irregular.

Figs 14



MOVEMENT OF SALT TOWER RELATIVE TO BALLAST

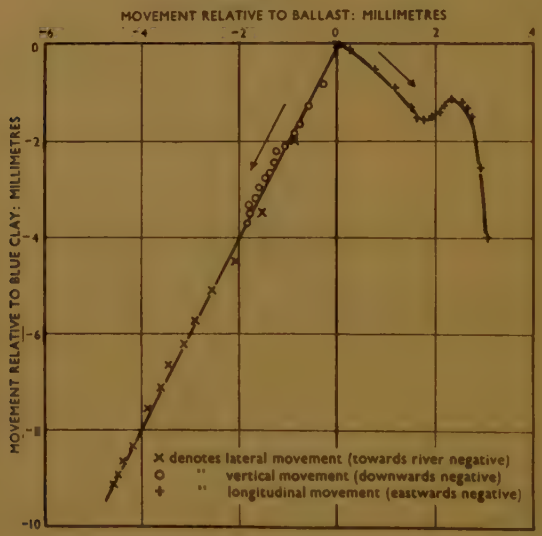
The movements indicated relative to the ballast are shown in *Figs 14*; in the lateral and vertical directions they have the same sense as those with respect to the blue clay, but are smaller.

It is known that since 1940, at least, some of the earth has fallen in on the eastern side of the shaft surrounding the two brick piers at the Salt Tower, and that this debris has been touching the pier that is built on the blue clay. Water in the pit hides the bottoms of both piers, and although the pier built on the ballast is clear of earth above the water level, it is not

known whether any earth has fallen so as to be in contact with this pier below the water level. Some doubt is therefore thrown upon the records of movement of the Salt Tower, the more so because of the discontinuities since about 1940 in the curves for lateral and longitudinal movement in *Figs 13* and *14*, without any corresponding discontinuity in the curves for vertical movement. There is also a possibility that the reference piers may have been slightly shifted by the earth tremors resulting from bombs which fell within the Tower precincts during the 1939-45 war. The shaft will, however, be cleared and re-faced as soon as possible.

In the following three paragraphs consideration is therefore restricted to observations taken before 1940.

Fig. 15



RELATION BETWEEN MOVEMENTS OF SALT TOWER RELATIVE TO BLUE CLAY AND BALLAST RESPECTIVELY (1926–1939 INCLUSIVE)

In *Fig. 15* the movements of the Salt Tower in the three directions—lateral, vertical, and longitudinal—with respect to the blue clay are plotted against the corresponding movements with respect to the ballast. The relationship between the two longitudinal movements is not simple, but both the lateral and the vertical movements follow approximately the same straight line on the graph.* The movement relative to the ballast is at any time about one-half of that relative to the blue clay. Thus the movement of the Salt Tower relative to the blue clay in a plane perpendicular

* The plotted points in *Figs 15*, *16*, and *17* do not represent direct experimental observations but are readings taken from the curves of *Figs 12*, *13*, and *14* at intervals of 1 year on the time scale.

o the bank of the river appears to be the simple sum of a movement relative o the ballast and an almost identical movement of the ballast relative to he blue clay.

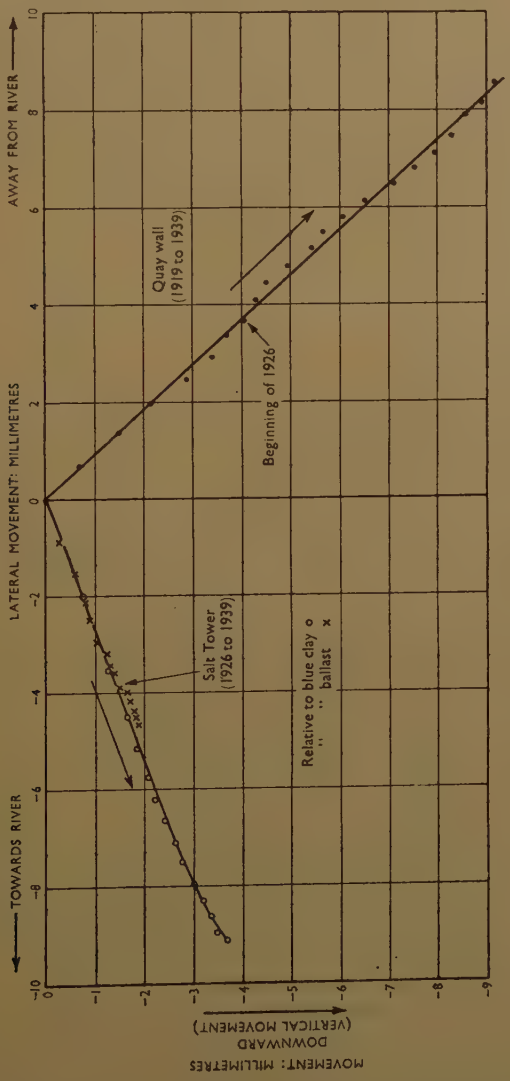


Fig. 16

Relation between Lateral and Vertical Movement (Quay Wall and Salt Tower)

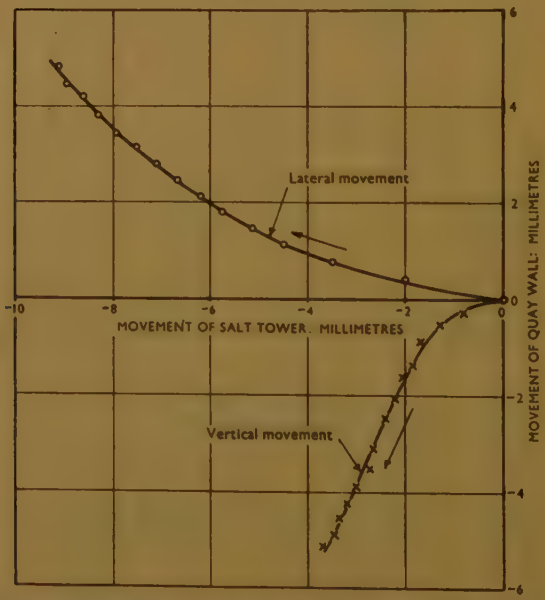
The direction of this movement can be seen in *Fig. 16*, where lateral movement is plotted against vertical movement to give a graph showing the direction of the total movement in the plane perpendicular to the bank

of the river. The graph also includes the results for the Quay Wall, and shows that while the Quay Wall moved relative to the blue clay (between 1919 and 1939) downwards and away from the river at an angle of about 45 degrees to the horizontal, the Salt Tower moved relative to both the ballast and the blue clay (between 1926 and 1939) downwards and towards the river at an angle of about 20 degrees to the horizontal.

Some indication of the relative rates of movement with respect to the blue clay is given by *Fig. 17*, which shows movements of the Quay Wall plotted against contemporaneous movements of the Salt Tower.

Measurements at a single station on a large block of masonry directly indicate, of course, only the movement at that station. In the absence of

Fig. 17



RELATION BETWEEN MOVEMENTS OF QUAY WALL AND SALT TOWER RELATIVE TO BLUE CLAY (LATERAL AND VERTICAL, 1926 TO 1936 INCLUSIVE)

cracks or other signs of relative movement within the block, however, it is reasonable to regard the measurements as a record of the movement of the block as a whole. For instance, the variation of the readings at the Quay Wall with tide and temperature fully accords with the expected behaviour of the Wall as a whole, and therefore it seems justifiable to regard the secular movement also as representing that of the entire structure.

Movement in Mass—Rochester Bridge

The measurements at Rochester Bridge, even after correction for temperature variation, directly represent only the movement across the expansion joint on the Strood pier, and the true point of origin of the movement is not determined by the readings. In particular, it cannot be inferred that the measurements do actually describe the movement of the Strood abutment. Nevertheless there are other distinct signs—such as cracks in the masonry—of movement of the Strood abutment, and there is no evidence of movement in any other part of the structure. Therefore it seems justifiable on the present evidence to regard the records as describing the movement of the Strood abutment, and adoption of this hypothesis at least avoids tedious circumlocution. As mentioned on p. 687, other methods are being tried to establish more definitely how the Strood abutment is moving.

Correction of Readings

The record of secular movement has been obtained by suitable correction of the actual readings, and the effect of the two successive corrections which are applied is illustrated, for a sample period of one year, in *Figs 18.*

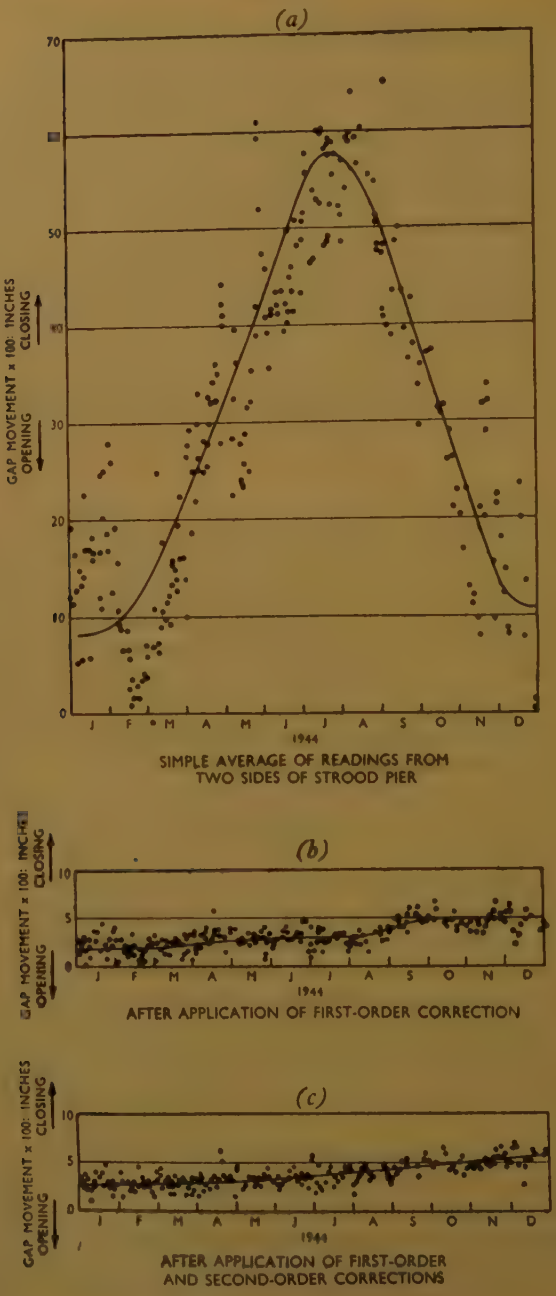
Fig. 18 (*a*) shows the simple average of the readings from the two sides of the Strood pier. There is a wide scatter, and a large variation from summer to winter. The effect of first-order correction is shown in *Fig. 18* (*b*), where the readings of *Fig. 18* (*a*) have had the corresponding readings on the Rochester pier subtracted from them. The scatter is much reduced, and the seasonal change is removed.

It was found possible, however, to carry the process of correction a stage further. The shade temperature near the bridge had been observed at the time of taking each set of readings. When sufficient values of movement to which the first-order correction had been applied were plotted against corresponding shade temperatures, the points were found to be grouped about a curve. (Due allowance had been made for the secular movement, which was already known approximately as a result of applying the first-order correction). A further empirical correction to the movement values could thus be made on the basis of the observed shade temperature ; it was only of the second order of magnitude, but it brought about a perceptible reduction in the scatter of individual readings, as is shown in *Fig. 18* (*c*).

Features of Secular Movement

The record of secular movement is shown in *Fig. 19* in the form of a mean line drawn through the points representing the observed values with the supplementary temperature correction applied as in *Fig. 18* (*c*). It shows an overall movement of 0·03 inch per annum towards the centre of

Figs 18



MOVEMENT AT ROCHESTER BRIDGE DURING 1944

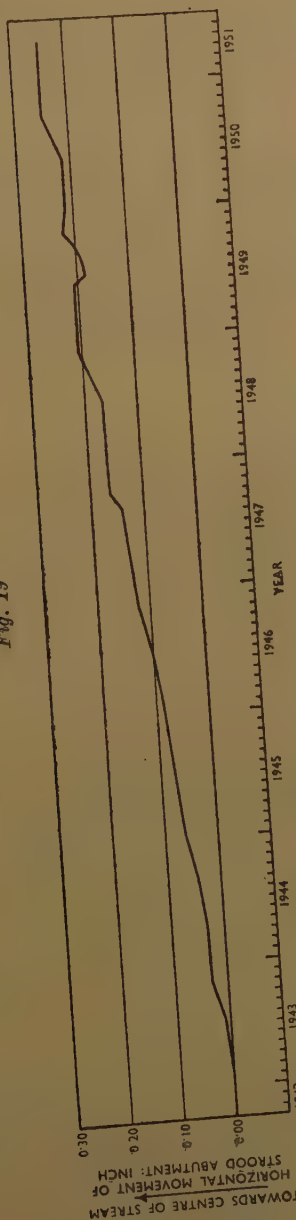


Fig. 19

SECULAR MOVEMENT AT ROCHESTER BRIDGE

the stream, but the rate is not constant throughout each year. During the winter and spring little or no movement occurs, and often practically the whole movement is concentrated in the late summer and autumn. Occasionally the movement towards the centre of the stream in late summer appears to be preceded by a slight movement in the opposite sense in early summer.

The reason for the increased rate of movement in the late summer and the autumn is not altogether clear, but may possibly be found in the autumnal rise of the groundwater level at the Strood bank, perhaps coupled with swelling of the soil composing the bank. The retaining wall of the bank would move outwards under such changes in the soil, and it is conceivable that its movements are transmitted to the Strood abutment through the invert and the girders spanning the small invert-channel.

Part 4.—Conclusion

The method of measurement described in the foregoing parts of this Paper has given a quantitative indication of movements which affect structures and which, though small, are different from the ordinary settlement that is familiar to the builder. The accuracy obtainable in measuring settlement by means of surveyors' levels is probably of the order of a few hundredths of an inch. The apparatus used at Waterloo Bridge [1] gave an accuracy of one or two thousandths of an inch, and the accuracy obtained with the Post Office Engineering Department instrument [2] was $\pm 0{\cdot}0015$ inch for indoor positions and $\pm 0{\cdot}003$ inch for outdoor positions.

The micrometer apparatus used at the Tower of London is sensitive to approximately $\pm 0{\cdot}0001$ inch, and in an indoor position where the total movement measured is small, the accuracy is reliable to almost as close a figure. When the total movement is larger, and when outdoor conditions have to be dealt with, there is a lower degree of accuracy.

The micrometer method would appear to be more accurate than any of the other methods just referred to. It possesses, indeed, a sensitivity which is not usually required in the measurement of the ordinary settlement of structures, and it would probably be rather troublesome to apply for that purpose.

Where, however, there is doubt concerning the stability of important structures, the method may serve the interests of safety and economy by affording a means to maintain a close watch on the structure and so to decide in good time if expensive repair work may become necessary.

Perhaps the most interesting feature of the records so far obtained is the evidence they afford of slow but steady movement of large masonry structures continuing over centuries. A certain amount of evidence of such movement is available from other sources. The Appendix is

summary of some of the points discussed in a Paper entitled " The Subsidence of London " by Capt. T. E. Longfield. In his Paper are described indications of a general sinking of the land in the London area with respect to the mean sea level at a rate which is of the order of 9 inches per 100 years.

Charles Darwin[7] described how the activity of earthworms in withdrawing soil from beneath large stones resting on the ground and casting it up on the surrounding area can cause the stones to sink, during the course of years, in relation to the ground about them. He also described a similar process causing the gradual burial of ruins of old buildings, such as Roman houses, when the foundations were not placed too deep for the worms to penetrate. In one place, it seems, he had observed walls completely covered with earth, although the foundations lay at a depth of 14 feet, and he remarked that the fact is difficult to account for. It is perhaps permissible to conjecture that the explanation lies in secular movements of the kind observed in the Quay Wall and Salt Tower at the Tower of London.

With regard to the magnitude of movement, although there is not a precise correspondence it can be said that the movements alluded to by Longfield and Darwin and those described in this Paper are all of the same order of magnitude, that is to say, some inches every 100 years. According to Darwin, earthworms can be responsible for the addition of as much as $\frac{1}{5}$ inch of mould to the surface of the soil every year, but the amount varies with the concentration of earthworms at the site being studied, and some of the deposit is no doubt removed by wind, rain, and other influences. Longfield mentions a rate of sinking of the London ground surface of about 9 inches per 100 years.

The average rate of sinking of the Quay Wall at the Tower with respect to the blue clay during the period of observations here described has been equivalent to rather less than 2 inches per 100 years.

The Quay Wall is about 700 years old, and there seems to be every reason to believe that the movement recorded during the last 30 years has continued over its whole life at an equal rate at least, and probably at a greater one ; it appears, therefore, that the Wall has sunk about 1 foot since it was built and that it has moved away from the river through a comparable distance.

Two further features are worthy of note. One is the evidence from the Salt Tower that there is relative movement of one stratum of the soil over another ; the other is the evidence from Rochester Bridge which indicates that the stability of the soil varies very greatly within short distances and under very slight changes of condition.

The opportunities which present themselves for measurements of the kind described in this Paper can only be few and the records must be continued for several years before they can reveal the true pattern of movement. The Authors and their colleagues at the National Physical

Laboratory have therefore been interested to maintain the two series of measurements in progress, in the hope that the continued records will help to reassure civil engineers and architects, who may in other buildings find slight but disquieting evidence of slow movement of masonry structures.

Acknowledgements

The work described was undertaken by the National Physical Laboratory for the Ministry of Works and the Rochester Bridge Trust, and the Authors' thanks are due to the Director of the Laboratory, to the Ministry of Works, and to the Court of the Bridge Wardens for permission to publish this account.

The Authors wish also to acknowledge the assistance afforded them by the staff of Messrs Howard Humphreys & Sons, previously consultants to the Bridge Wardens; by Mr L. W. A. Cate, the Bridge Caretaker; by the Superintendent of Works at the Tower, and by Mr Albert Eaton of the Laboratory staff, who for many years carried on the observation at the Tower.

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The Paper is accompanied by fourteen sheets of diagrams and six photographs, from which the Figures in the text and the half-tone page plates have been prepared, and by the following Appendix.

APPENDIX

ABSTRACT FROM
ORDNANCE SURVEY PROFESSIONAL PAPER.—NEW SERIES, NO. 14 (1932)

(" *The Subsidence of London,*" *and from other relevant investigations, by Captain T. E. Longfield, R.E.*)

This Paper mentions six different varieties of land movement which might possibly affect London. They are :—

1. A general and gradual lowering of the land surface in the south-east of England in relation to the mean sea level.
2. In addition to this, a rather more local but at the same time a general subsidence in the metropolitan area.
3. A seasonal subsidence and uplift caused by change of water content in the London clay (it should be noted that there is approximately 4 months' lag in the action of the clay).
4. Subsidence caused by the draining or pumping of water and sand from the gravel.
5. Rhythmic land movement synchronous with the rise and fall of the tide.
6. Possible subsidence due to underground tunnelling.

Since records of levels do not go back more than 70 years, geological evidence is used to supplement direct evidence from levelling.

Borings have disclosed in the buried channel of the Thames the fact that the surface of the land in the Neolithic Age is now about 60 to 70 feet below mean sea level. If it be assumed that the period involved is about 10,000 years, this represents about 9 inches per 100 years. Roman relics in Southwark suggest that this district was a well inhabited area in Roman times. It is now, however, 5 to 6 feet below Trinity high water mark, and, therefore, since the Romans would not choose a locality subject to flooding and accompanying disadvantages of a low level, it seems likely that the whole surface has sunk several feet since Roman times. It is possible, though not certain, that the severe floods of 1929 were the result not entirely of an exceptionally high tide but partly of land sinkage also.

The first geodetic levelling of Great Britain, carried out between 1840–60 with a datum in the form of a mark on the Old Dock Sill at Liverpool, was the basis of all Ordnance levels and contours. In 1921 a second levelling, with its datum as mean sea level at Newlyn, in Cornwall, was completed. A comparison of the old and new apparent heights indicated a difference increasing more or less proportionately to the distance south-east from Liverpool. The maximum was about 2 feet in Essex and there was a difference of about 1·25 foot in Kent. Observations have also been taken for a short period at Felixstowe, where an apparent sinking of the land with respect to mean sea level of 1·2 foot per 100 years has been indicated.

The Neolithic and the Roman evidence in the London district both indicated a rate of sinking of about 9 inches per 100 years, suggesting that the apparent rate of sinking in the south-east of England has been partly genuine and partly due to cumulative error in the old work. The Paper gives diagrams (reproduced by Wilson and Grace [8]) showing the results of levelling along lines traversing London, with checking by a number of cross-lines between these main radial lines. The diagrams all show an apparent sinking in the London area greater than that in the outer districts, and it is interesting that the subsidence seems to begin where the clay of the outlying districts changes to the gravel which occupies most of the central districts.

It is tentatively suggested in the Paper that this behaviour would be explained by the fact that the central part of the London area is almost entirely waterproofed by buildings, roads, and pavements ; the gravel beneath might, therefore, be drained, and the usual behaviour of gravel when drained is to lose sand which is carried away with the water, and consequently to suffer a subsidence.

It is mentioned that although the clay did not appear to have undergone any general subsidence, there is no doubt that clay is troublesome as a base for buildings because of the seasonal subsidence and uplift which result when the water content changes.

In the James Forrest Lecture, 1939, Dr Karl von Terzaghi [9] drew attention to

the settlement of clay strata due to the lowering of the underground water level, of which he had observed several instances. Guthlac Wilson and Henry Grace have remarked[8] that London rests on a thick bed of clay overlying chalk from which water is being pumped in considerable quantities and that, as a result, the artesian water level in the chalk has been falling steadily throughout the last century. On the basis of evidence of this fall extracted from Memoirs of the Geological Survey and other sources, Wilson and Grace applied Terzaghi's theory to the London clay, using data on the properties of the clay determined by Cooling and Skempton,[10] and they showed that the underdrainage effect could account for the whole movement recorded by Colonel Longfield in areas where the clay is thick and which are not heavily built up. In the central area round the Bank of England, where the clay strata are relatively thin, the rate of settlement is very much greater, and Wilson and Grace considered this to be consistent with settlement under the weight of the heavy buildings in this area.

Longfield also mentions that there is definite evidence of rhythmic land movement due to the rise and fall of the tide. A seismograph in the County Hall, Westminster, has shown a definite movement of the building corresponding with high and low tides. The old Waterloo Bridge sank at high tide and rose again at low tide owing to the compression of the weight of the water. Some underground railway tunnels below the Thames, which are usually circular in section, become very slightly elliptical as a result of the weight of the water above them at high tide, and regain their normal shape when the tide recedes.

There seems to be no evidence to connect underground tunnelling with the fairly general subsidence in the London area, although a few local subsidences have occurred at the actual time of the construction of certain tunnels.

Paper No. 5863

"Pressure of Concrete on Formwork"

by

Stanley Rodin, M.Sc., B.Sc.(Eng.), A.M.I.C.E.

(Ordered by the Council to be published with written discussion)†

SYNOPSIS

The Paper reviews the published experimental data on the lateral pressure of concrete against forms, and discusses the various factors that affect the lateral pressure. These are (1) the rate of filling the forms, (2) the method of placing the concrete, whether by hand or vibration, (3) the consistency and proportions of the mix, (4) the temperature of the concrete, (5) the rate of setting of the concrete, and (6) the size and shape of the form.

The amount of published data is very limited and the reliability of much of it is doubtful. The Paper presents a rational explanation of the physical phenomena causing the type of pressure distribution found in practice, and shows why this is not hydrostatic, except in special circumstances.

It is shown that, in the design of forms, lateral pressures lower than those normally recommended can generally be used. Curves are given, which enable the lateral pressures on the forms to be estimated from a knowledge of the rate of pour and the characteristics of the concrete.

Introduction

The cost of formwork may represent as much as 30 to 40 per cent of the total cost of concrete structures, and any saving in the cost of forms, for example by a reduction of design loads, would represent an important item. The purpose of the Paper is to review the work that has been done to determine the lateral pressure of concrete against forms, and, from this data, to see whether a rational design procedure is at present possible.

Factors Influencing Lateral Pressure

The factors most likely to affect the pressure developed by fresh concrete are generally believed to be :—

(1) Rate of filling the forms.
(2) Method of placing the concrete, whether by hand or by vibration.
(3) Consistency and proportions of the mix.
(4) Temperature of the concrete.

† Correspondence on this Paper should be received at the Institution by the 1st March, 1953, and will be published in Part I of the Proceedings. Contributions should be limited to about 1,200 words.—Sec. I.C.E.

(5) Rate of setting of the concrete.
(6) Size and shape of the form.

The Author has analysed the published experimental data in an effort to determine the relative influence of each of these factors. Before reviewing the data in detail, the methods used by the various investigators to measure the lateral pressure of concrete against forms will be discussed, since they control the reliability of the data.

Methods of Measuring Form Pressures

In the tests carried out by Roby [1] the concrete pressure was measured by the deflexion of a $\frac{7}{16}$-inch steel plate, 6 inches wide, extending the full width of the form and resting on knife edges 28 inches apart. The steel plate was located near the bottom of the form. By means of a pivoted bar, connected to a lever to give a ratio of 10 : 1, the deflexion of the centre of the plate was read on a scale graduated in $\frac{1}{64}$ inch, and by using a magnifying glass it was possible to read to within $\frac{1}{256}$ inch. This corresponded to a plate deflexion of $\frac{1}{2560}$ inch (0·0004 inch). The thickness of the timber sheathing above and below the steel plate was designed to give the same deflexion under load as the steel plate. No data is given as to whether this was checked during the tests. During the tests the maximum pressure developed was 1,000 lb. per square inch, which corresponded to a plate deflexion of 0·237 inch. The magnitude of the deflexion was quite appreciable and might have had an effect of relieving the steel plate of some pressure.

Shunk [2] inserted a 9¼-inch-diameter cylinder, fitted with a freely moving piston, into the wood formwork, and by means of a weighted lever arm the load required just to prevent the piston from moving under the applied concrete pressure could be determined. Generally, it was necessary to move the piston in and out about ¼ inch, until the observer felt it to be "alive." It is doubtful if, with such large movements, and since the piston appears to have been rather crudely fashioned, any reliance can be placed on the test results.

McDaniel and Garver [3] used a pressure gauge consisting of a small flexible German-silver diaphragm acting upon a reservoir containing mercury. The reservoir was connected to a glass tube, and the pressure was indicated by the position of the mercury in the tube. No mention is made of the order of the movement of the diaphragm under the applied concrete pressure.

Macklin [4] calculated the pressure of concrete against the formwork by measuring the deflexion of the wood sheathing produced by these pressures. The deflexion of the sheathing relative to the supporting studs was measured by means of a dial-type micrometer mounted on a bridge arrangement.

[1] The references are given on p. 745.

The pressure cell used by the California Division of Highways[5] consisted of a metal disk, to one side of which a sheet-rubber diaphragm was clamped in a manner similar to that of a drum head. A shallow space between the rubber diaphragm and the disk was filled with liquid, which operated an ordinary pressure gauge mounted on the back of the disk. The cells, 6 inches and 12 inches in diameter, were inserted into the form wall so that the rubber diaphragm was flush with the inside surface of the wall, and care was taken to extract all air from the pressure cell. No indication is given of the volume change undergone by the cell when the concrete pressure was applied.

Davis, Jansen, and Neelands,[6] during the restoration of the upstream surface of the Barker Dam with Prepakt concrete, attached strain meters to the 1-inch-diameter steel bars which anchored the pre-cast face slabs to the face of the old dam. The stresses in the anchor bar were calculated and the pressures against the slab form, during the process of filling with aggregate and the later process of grouting, were determined. The strain gauges were Carlson electric-resistance strain meters, and were welded to the anchor bar so that they formed part of the bar. Experience with the Carlson strain meters has indicated their general reliability.

To measure the pressure of underwater concrete, placed by tremie, Halloran and Talbot[7] placed Waterways Experiment Station-type pressure cells in the forms, flush with the concrete surface. The movement of the diaphragm of the cell, resulting from the applied pressure, was of a very small order, and, in general, the results may be considered reliable. Similar pressure cells were used by the Bureau of Yards and Docks, Navy Department, for their tests on tremie-placed concrete.[8]

Smith,[9] Slater and Goldbeck,[10] and Teller[11] all used Goldbeck-type pressure cells inserted in the forms with the diaphragm flush with the inside surface. The working of this type of cell requires a small movement of the diaphragm against the concrete to break the circuit. The effect of this motion may have an important bearing on the pressure indicated by the cell, because it acts against the passive resistance of the concrete, which may be greater than the static pressure which existed prior to the movement. It is assumed that if the movement is small, the pressure required to break the contact is equal to the static concrete pressure. Data quoted by Seaquist[12] and Goldbeck[13] indicate that the counter-movement required to break contact was from 0·002 to 0·0004 inch for an applied pressure of about 7 lb. per square inch. This motion is likely to have a serious effect in dense granular materials, such as concrete, where it will probably aid arching and hence show erroneous results.

From the foregoing it can be seen that the methods used by various investigators for measuring form pressures varied considerably. In many cases, owing to the methods adopted, considerable doubt is thrown upon the accuracy of the results, and hence upon the reliability of many of the conclusions drawn from them.

Size and Shape of Forms

The types and principal dimensions of formwork used by the various investigators reported in the Paper are shown in Table 1.

Table 1

Investigator	Size and shape of form
Roby [1]	15 foot high × 2 foot 6 inch square columns
Smith [9]	10 foot high × 9¼ inch × 8 inch columns
,,	4 foot 8 inch high × 11 foot 5 inch × 10 inch wide wall blocks
,,	6 foot 6 inch high × 18 inch wide wall
Slater and Goldbeck [10]	16 foot 11 inch high × 2 inch wide walls
MacMillan [14]	*
McDaniel and Garver [3]	15 foot high × 23 inch × 2 foot 6 inch columns
Teller [11]	12 foot high × 2 foot × 8 inch columns
Maxton [15]	*
Portland Cement Association [16]	*
Davis, Jansen, and Neelands [6]	160 foot high × 3 foot to 8 foot wide wall
Davis and Davis [18]	2 foot to 7 foot high × 2 foot × 6 inch wide wall blocks
,, ,,	Up to 9 foot high columns
Stanton [5]	15 foot high × 18 inch to 26 inch wide wall
,,	12 foot high × 3 foot wide wall
,,	6 foot square column
Halloran and Talbot [7]	48 foot high × 20 foot wide dock walls
Bureau of Yards and Docks [8]	29 foot high dock walls
Hoffman [19]	3·38 foot high × 3·28 foot × 7·0 foot column
Macklin [4]	10 foot high × 9 inch wide walls.

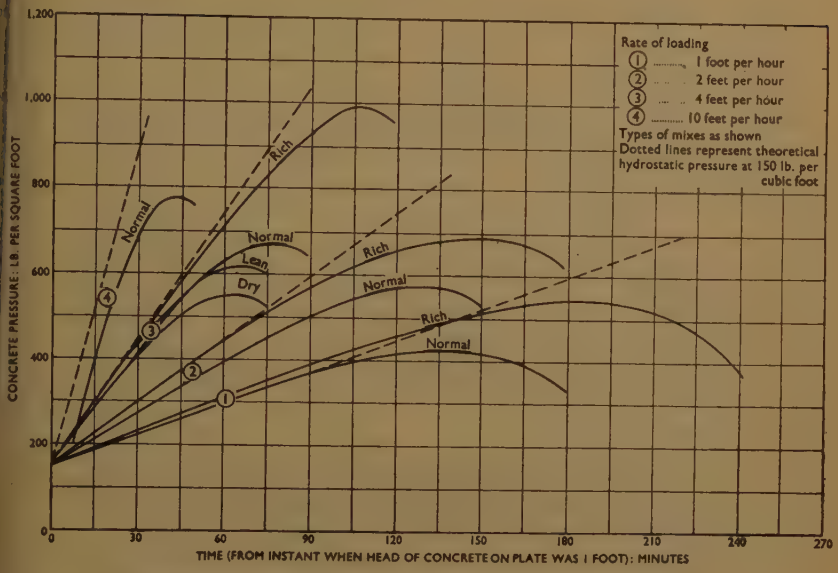
* Indicates data not published.

Rate of Filling Forms

(1) *Concrete Placed by Hand*

In the tests reported by Roby,[1] the rate of filling the square column was varied between 1 and 10 feet per hour. The results for typical mixes are shown in *Fig. 1*, and are summarized in Table 2.

Fig. 1



EFFECT OF RATE OF FILLING FORMS ON PRESSURE (ROBY)

TABLE 2

Type of Mix	Rate of filling	Maximum concrete pressure	Time to reach maximum pressure
Normal	1 foot per hour	420 lb. per square foot	3 hours
,,	2 ,, ,,	570 ,, ,,	2½ ,,
,,	4 ,, ,,	670 ,, ,,	1½ hour
,,	10 ,, ,,	780 ,, ,,	¾ ,,
Rich	1 foot per hour	540 lb. per square foot	4 hours
,,	2 ,, ,,	690 ,, ,,	3 ,,
,,	4 ,, ,,	1,000 ,, ,,	2 ,,

These tests indicate clearly that the higher the rate of filling, the greater will be the maximum pressure on the form.

The results of laboratory and field tests made by Smith[9] are summarized in Table 3. They show that the maximum pressure exerted on the forms, and the head of concrete at the point of maximum pressure, both increase as the rate of filling increases.

Slater and Goldbeck[10] measured the horizontal pressure exerted by concrete against the forms, while pouring part of a reinforced-concrete ship section. The concrete was made of a 1 : 2 : 1⅓ mix. The few readings

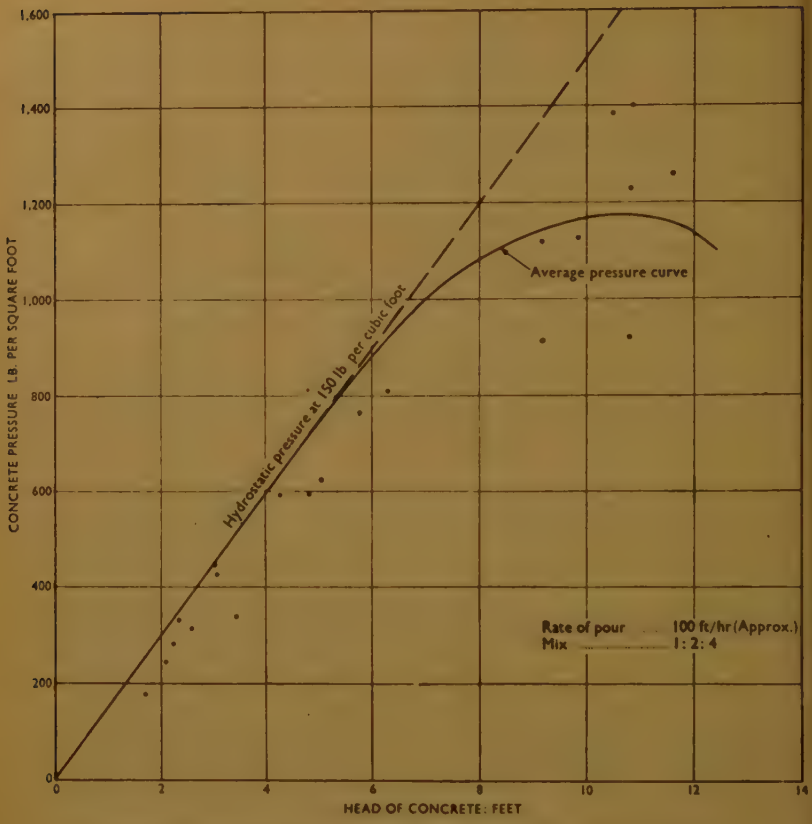
TABLE 3.—TESTS OF PRESSURES OF CONCRETE AGAINST FORMS. (SMITH)

Laboratory Tests

Mix	Consistency	Air temp: °F.	Rate of pouring: ft. per hr	Maximum pressure: lb. per sq. ft		Head at maximum pressure: feet		Time at maximum pressure: minutes	
				Vertical	Lateral	Vertical	Lateral	Vertical	Lateral
1 : 1½ : 3	3″ slump	62	1·8	340	170	3·0	2·5	75	75
,,	Quaky	70	1·8	400	270	4·0	3·5	106	106
,,	,,	68	3·8	500	350	4·8	4·3	66	66
,,	,,	64	7·5	660	450	7·1	6·6	60	60
,,	8½″ slump	64	7·6	480	690	4·8	4·3	38	38
1 : 2 : 4	Quaky	68	1·7	330	500	3·5	2·7	96	96
,,	9½″ slump	65	1·8	400	230	3·4	2·9	85	85
,,	10½″ slump	77	1·8	250	140	2·1	1·9	46	56
,,	Quaky	76	3·6	340	210	4·5	3·4	56	56
,,	,,	74	7·3	250	170	4·5	4·0	37	37
,,	11½″ slump	67	7·4	350	280	3·3	2·8	28	28
,,	9½″ slump	81	7·4	280	170	3·3	2·8	27	27
1 : 2¼ : 3¾	—	74	1·5	170	100	2·5	2·0	90	90
,,	—	80	2·7	210	130	3·7	2·5	78	61
,,	—	—	3·3	240	110	3·3	2·5	49	44
1 : 3 : 6	Quaky	70	1·7	140	130	1·7	2·1	36	66
,,	,,	66	1·6	—	80	2·8	1·7	79	61
,,	,,	59	3·3	150	60	4·1	2·5	70	50
,,	,,	64	3·5	290	120	3·7	3·5	57	57
,,	,,	66	6·7	150	30	6·6	3·8	57	37
,,	,,	62	7·1	360	200	5·5	5·0	49	49
,,	,,	63	9·0	120	50	9·3	8·8	10	10
		63	14·3	190	60	10·0	5·1	48	29

Mix	Consistency	Air temp: °F.	Rate of pouring: ft. per hr	Maximum pressure: lb. per sq. ft		Head at maximum pressure: feet		Time at maximum pressure: minutes	
				Vertical	Lateral	Vertical	Lateral	Vertical	Lateral
1 : 2 : 4	Sloppy	74	9·0	—	320	—	3·0	—	20
,,	,, ...	74	10·6	—	210	—	2·3	—	13
,,	,, ...	57	12·0	—	280	—	3·2	—	17
,,	,, ...	57	12·0	—	350	—	3·2	—	17
,,	,, ...	74	12·5	—	270	—	2·3	—	11
,,	,, ...	57	20·0	—	350	—	4·6	—	23
,,	,, ...	57	20·0	—	560	—	4·6	—	23

Fig. 2



PRESSURE OF CONCRETE AGAINST FORMS (McDANIEL AND GARVER)

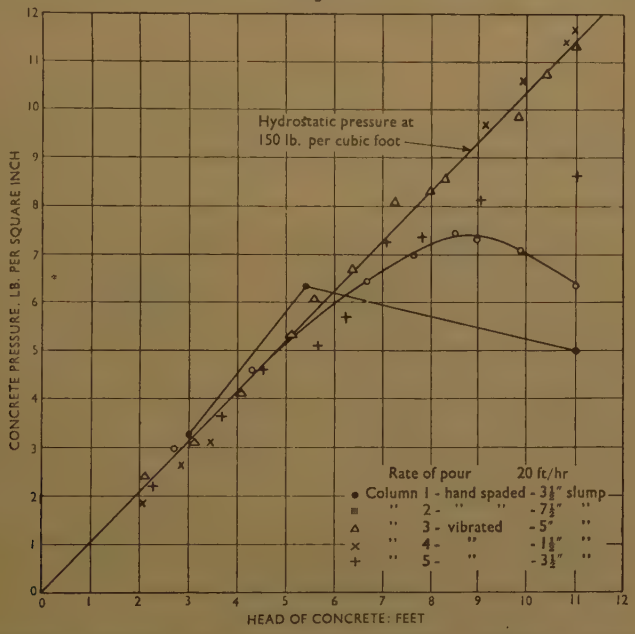
taken indicated that at a pouring rate of about 6 feet per hour, the maximum pressure against the forms was 460 lb. per square foot, owing to the head of concrete at the end of 40 minutes of pouring. After this time the pressure gradually decreased in spite of the increasing head of concrete.

MacMillan [14] states that "tests made by the Aberthaw Company of Boston, and the results of field experience show that the pressure against forms should be figured on the basis that the first concrete is a liquid weighing 145 lb. per cubic foot. This pressure continues to build up as additional fresh concrete is added until a time period of approximately 30 minutes has elapsed. After this time there seems to be no further increase in the maximum pressure. Obviously the strength of cleats, boards, ties, and other parts of forms for walls and columns must be considered in relation to the depth of concrete likely to be placed in 30 minutes. Concrete

placed during cold weather, unless the construction is enclosed and kept warm, continues to build up pressure for a period considerably longer than 30 minutes." No other data are given.

Fig. 2 shows the results of some field tests made by McDaniel and Garver,[3] on 23-inch-by-30-inch posts containing reinforcing steel. A 1 : 2 : 4 concrete of "wet" consistency was used; it required 7 to 9 minutes to fill one of the posts, which is equivalent to a rate of pour of about 100 feet per hour.

Fig. 3


Hydrostatic pressure at
150 lb. per cubic foot
CONCRETE PRESSURE, LB. PER SQUARE INCH
HEAD OF CONCRETE: FEET
Rate of pour 20 ft/hr
Column 1 - hand spaded - 3½" slump
" 2 - " " - 7½" "
" 3 - vibrated - 5" "
" 4 - " - 1½" "
" 5 - " - 3½" "

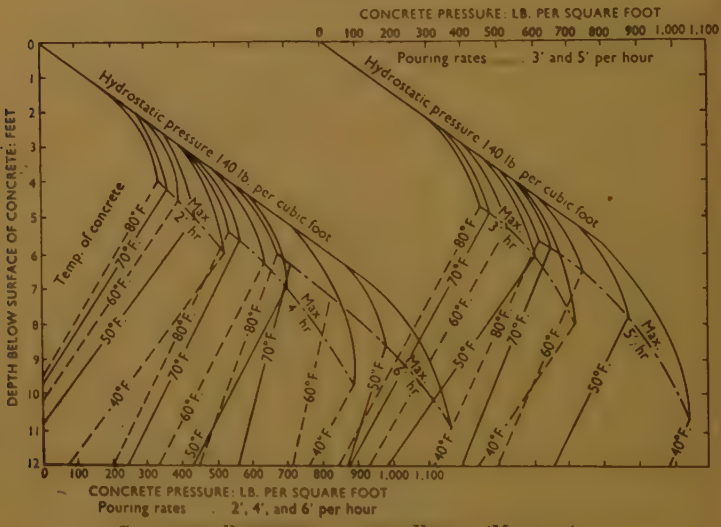
CONCRETE PRESSURE FOR SPADED AND VIBRATED CONCRETE (TELLER)

The results of some tests made by the Bureau of Public Roads were reported by Teller.[11] *Fig. 3* shows the concrete pressure plotted as a function of the head of concrete during the pouring of the columns, the pressure readings being for the lowest cell, which was located 1 foot above the base. The concrete was a 1 : 2 : 4 mix and weighed 150 lb. per cubic foot, and the rate of filling was approximately 20 feet per hour; the temperature at the time of placing varied between 49° and 62° F.

Maxton[15] presented a set of pressure curves (see *Fig. 4*) to be used for designing forms. Basic data for the upper portions of these curves, above the points of maximum pressure, were taken from tables compiled from tests made by the United States Army Engineers. (No details or other test data are given.) The hydrostatic pressure line was reduced,

however, from 150 to 140 lb. per square foot, to make the data applicable to low-slump concrete used in large forms. At the point of maximum pressure, the Army Engineers' data were abandoned (no reason being given for this) and the curves were completed by using data obtained from curves published in the Portland Cement Association's publication "Forms for Architectural Concrete." The latter gave pressures only for concrete placed at 70° F. and at 50° F., and the remaining curves were interpolated. Maxton considers that the pressures given in *Fig. 4* are ample for vibrated concrete in widely spaced forms such as are used in block construction on locks and dams. The curves may also be used where high-slump concrete

Fig. 4


CONCRETE PRESSURE: LB. PER SQUARE FOOT
0 100 200 300 400 500 600 700 800 900 1,000 1,100
Pouring rates 3' and 5' per hour
Hydrostatic pressure 140 lb. per cubic foot
Hydrostatic pressure 140 lb. per cubic foot
DEPTH BELOW SURFACE OF CONCRETE: FEET
Temp. of concrete
80°F.
70°F.
60°F.
50°F.
40°F.
Max. 2' hr
Max. 3' hr
Max. 4' hr
Max. 5' hr
Max. 6' hr
CONCRETE PRESSURE: LB. PER SQUARE FOOT
Pouring rates 2', 4', and 6' per hour

Concrete Pressure against Forms (Maxton)

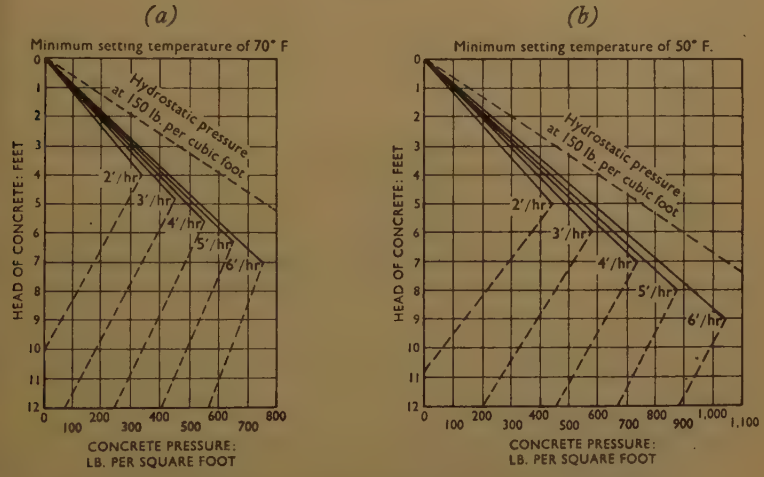
is placed without vibrators in narrow forms. Where narrow forms, vibrators, and high rates of pour are used, a pressure equal to the hydrostatic pressure of the concrete should be used for the full depth of the forms.

The latest design curves published by the Portland Cement Association[16] are presented in *Figs 5*, which are based on the results of tests made by the Universal Form Clamp Company on hand-placed concrete. Five rates of placing concrete, ranging from 2 feet to 6 feet per hour, are shown, these rates being normally used for wall construction. Forms above the points of maximum pressure are to be designed to withstand pressures indicated by the solid lines. Forms below points of maximum pressure are to be designed to withstand the maximum pressures indicated for the various rates of fill. For more rapid placing, as might occur in columns, it is recommended that a hydrostatic pressure equivalent to that resulting

from a liquid weighing 125 lb. per cubic foot should be used, as stated by Wynn.[17] In neither of these two publications are any other test data presented.

Davis, Jansen, and Neelands[6] have presented the results of form-pressure measurements taken during the restoration of the upstream face of Barker Dam with Prepakt concrete. *Fig. 6 (a)* shows the variation in form pressures, for several cell locations selected at random, caused by the aggregate. *Fig. 6 (b)* shows the maximum lateral pressures produced by the aggregate during the entire process of filling the forms. In general, the authors consider that the lateral forces produced by the aggregates were

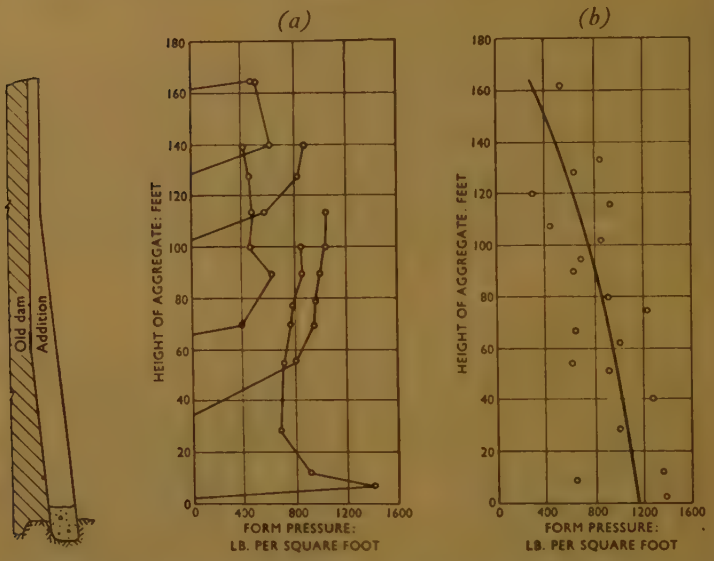
Figs 5



Concrete Pressure against Forms (Universal Form Clamp Co.)

considerably greater than those which might be expected from a granular material placed without vibration, and that the variations in pressure, as indicated by different meters, were large. Possible causes of the erratic form pressures were: (1) the effect of the daily variations in temperature on the anchor-bar steel to which the strain meters were attached; and (2) the method of pouring, which might have produced local vibrations. No information is given regarding the rate of pouring. During the period of grouting, at an average rate of 20 feet per day, the increase in form pressure contributed by the grout was so small as to be practically negligible. Since, under the low-temperature conditions at Barker Dam, the grout (having an initial temperature of about 50° F.) remained in a plastic state for a period of not less than 24 hours, and therefore might by itself be expected to create full liquid pressure for this period of time, it appears, according to the authors, that the laws governing lateral pressures of freshly

Fig. 6



GRAPHS SHOWING (*a*) VARIATION IN FORM PRESSURE DUE TO AGGREGATE; (*b*) MAXIMUM FORM PRESSURES CAUSED BY AGGREGATE (DAVIS, JANSEN, AND NEELANDS)

grouted aggregate masses are at variance with those which apply to the grout alone.

(2) *Concrete Placed by Vibration*

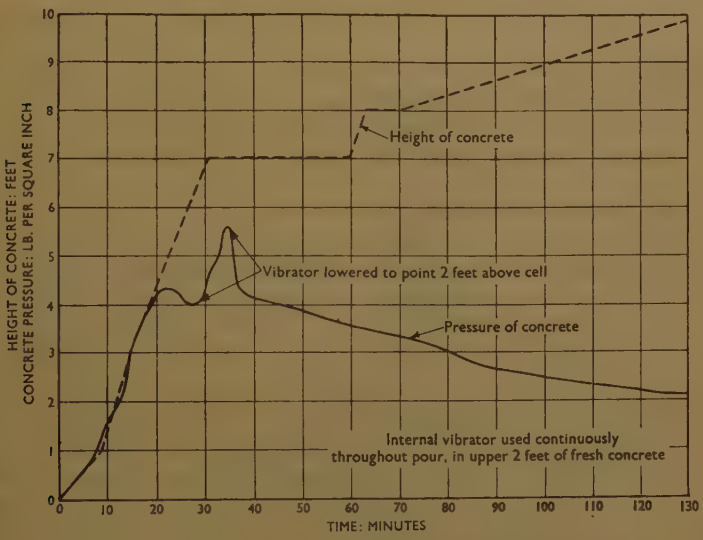
A limited amount of data concerning the effect of vibration on the pressure of concrete against deep forms was obtained from tests made by the Bureau of Public Roads and reported by Teller.[11] The vibrator consisted of an electric motor, operating at a frequency of 3,600 vibrations per minute, attached to the outside of the form. The results obtained are shown in *Fig. 3*, which compares the effects of vibration and hand-spading. In the case of the vibrated concrete the fluidity was retained throughout the depth of pour, even for the concrete having a slump of only 1½ inch.

In presenting their conclusions of an investigation to determine the effect of vibrated concrete on form pressures, Davis and Davis[18] state: "During the period of vibratory tamping, the pressure of fresh concrete against the form, for that portion of the mass which is in agitation, is substantially the same as that which would be produced by a liquid having the same density as the concrete. This is true regardless of the consistency of the mix. After the cessation of vibration, however, the pressure against the forms drops off rapidly, until in a few minutes it is

approximately the same as that produced by hand-tamped concrete under ordinary conditions. Under conditions encountered in building construction, with the limitations there obtained as regards rate of pour and mass of concrete to be vibrated, the maximum form pressures may be expected to be of the order of perhaps 3 to 4 lb. per square inch." No details of the test results upon which this conclusion was based are given.

Stanton[5] reported the results of a series of tests made by the California Division of Highways to ascertain the effect of an electrically-driven internal vibrator on concrete pressures. Measurements were made on five different structures. The first structure was a retaining wall 15 feet

Fig. 7



PRESSURE DEVELOPED BY VIBRATED CONCRETE (STANTON)

high, 26 inches wide at the bottom, and 18 inches wide at the top, the pressure cell being placed 4 feet above the footing. *Fig. 7* shows the relationship between the height of the concrete above the cell and the recorded pressure. Except in the instance noted in the diagram, the vibrator was embedded in the upper 2 feet. The readings indicated that the full hydrostatic pressure acted until the head of concrete was 4·3 feet, after which the concrete pressure steadily decreased, except when the vibrator was lowered through the concrete to a point about 2 feet above the cell for a period of 2 minutes. The second and third tests were made during the placing of a wall 3 feet thick and 12 feet high ; the fourth on a battered wall similar to that in the first test ; and the fifth on a large

column approximately 6 feet square, in which the concrete was placed rapidly. The results were as shown in Table 4.

TABLE 4

Test No.	Limit of hydrostatic pressure		Maximum head of concrete: feet	Remarks
	Pressure: lb. per sq. in.	Concrete head: feet		
1	4·3	4·3	10	No further increase in pressure
2	6·0	7·0	—	,, ,, ,,
3	5·3	7·0	11	,, ,, ,,
4	4·5	4·5	13	,, ,, ,,
5	6·2	6·5	9·5	Maximum pressure of 6·7 lb./sq. in. at 9·5 feet

All the concretes were designed to have a slump of 2 inches, but some differences in workability were noted. No information is given on the concrete mixes used in these tests. It has been assumed that in tests Nos 2 to 5 the rate of pour was 14 feet per hour as in test No. 1.

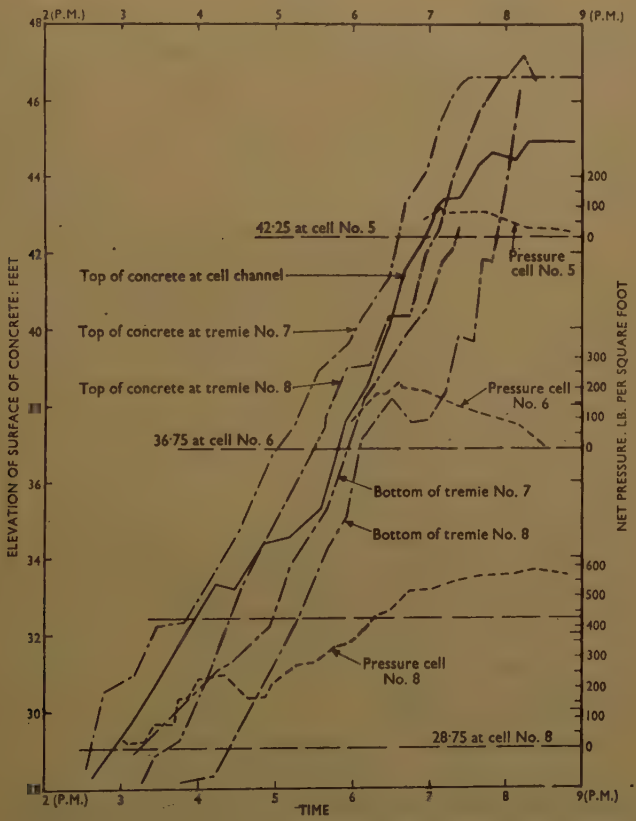
(3) *Concrete Placed Under Water by Tremie*

A study of the behaviour of concrete placed by tremie reported by Halloran and Talbot[7] included measurement of form pressures by W. E. S.-type pressure cells. It was found that the form pressures were lower than had been assumed (presumably the equivalent hydrostatic head). In general, the pressures continued to build up to a maximum until the concrete reached a head of about 10 feet above the cell, after which the pressures reduced. With some cells, for example No. 8, the pressure stopped decreasing after a short time, and then continued to increase again over a long period of time. Vibration, location of the tremie, and elevation of the tremie with respect to the cells affected the pressure. Stiffening of the concrete, whether the result of hardening because of lapse of time, or of rapid-setting action of the cement, or temperature of the concrete, materially affected the flow of the concrete and the transmission of pressure. The flow of the concrete from the tremie towards a cell caused the building up of pressures greater than those caused by static head, even through several feet of concrete. It was also found that the pressures were reduced materially after the bottom of the tremie was raised above the elevation of the pressure cell. *Fig. 8* shows: (1) the elevation of the concrete with respect to time; and (2) the elevation of the bottom of the tremie for each elevation of the concrete. Superimposed on these curves is a pressure curve for each cell on which the pressure is plotted against time. The net pressures were obtained by subtracting the full hydraulic pressure from the indicated cell pressure. From the limited data presented by the authors, that for cell No. 8 shows the highest concrete pressures, as follows:

Maximum net concrete pressure = 575 lb. per square foot.
Concrete head at maximum pressure = 17·5 feet.
Rate of pour = 4 feet per hour.

The concrete mix contained seven sacks of cement per cubic yard of concrete and a water/cement ratio of 5½ gallons per sack, giving a slump of 7 inches.

Fig. 8

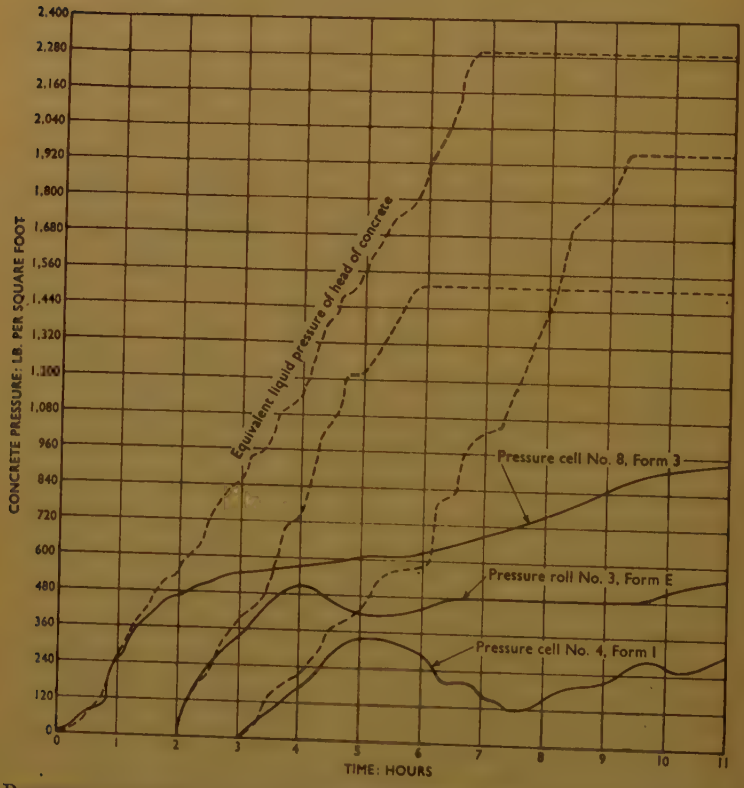


Pressure of Tremie-Placed Concrete (Halloran and Talbot)

During the construction of a dry-dock, the Bureau of Yards and Docks[8] measured the lateral pressure exerted on the forms during tremie pouring, by means of the W. E. S.-type pressure cells. Two tests were made, using rates of pour of 2·75 and 3·8 feet per hour, the concrete containing seven sacks of cement per cubic yard and having a slump of 6½ inches. Some typical pressure curves are shown in *Fig. 9*. A summary of the results

showed that, at first, the lateral pressures were closely related to the equivalent liquid pressure head of the (submerged) concrete, and then reduced by an amount ranging from 10 to 65 per cent of the equivalent liquid pressure head of the concrete. The maximum lateral pressure measured during the period of initial setting was 500 lb. per square foot, under a head of concrete of nearly 8 feet above the height of the cell and

Fig. 9



Pressure of Tremie-Placed Concrete (Bureau of Yards and Docks)

2 hours after placing. The equivalent liquid pressure head of the tremie-placed concrete was 625 lb. per square foot. (Allowing for the submerged weight of the concrete, this is equivalent to a pressure in air of $500+(8\times64)=1{,}010$ lb. per square foot.) Some cells indicated larger and gradually increasing pressures for several hours after the completion of pouring.

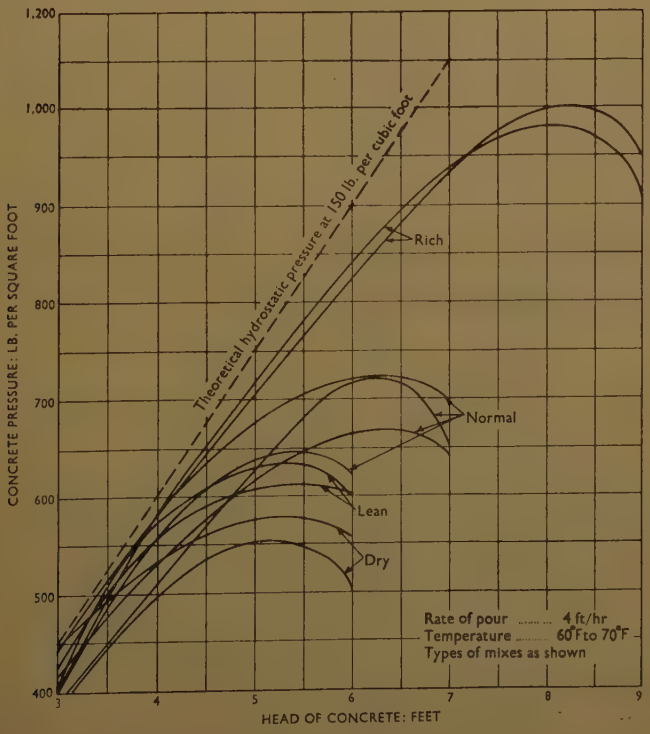
Consistency and Proportions of Mix

To determine the influence of cement and water content on form pressures, Roby[1] made a series of tests using the concrete mixes shown in Table 5.

Table 5

	Normal	Dry	Rich	Lean
Proportions	1 : 2 : 3½	1 : 2 : 3½	1 : 1¼ : 2¼	1 : 2½ : 5
Average slump	7 in.	3 in.	7 in.	7 in.
Water/cement ratio	0·91	0·86	0·66	1·10
Weight : lb./cu. ft	151	151	150	154
Design strength : lb./sq. in.	2,400	2,700	3,800	1,700

Fig. 10



Effect of Consistency on Form Pressure

Fig. 10 shows the influence of consistency of the mix, when placed at the rate of 4 feet per hour. The cement content had a large effect on the form pressures, the rich mixes developing pressures 40 per cent greater than the normal mix, which in turn developed pressures of about 15 per cent more than the lean mix. The influence of water content is clearly shown by the dry mix, which developed a maximum pressure of about 20 to 25 per cent less than the normal mix.

The results of the tests reported by Smith [9] (see Table 3) also show that the richer the mix the greater is the maximum pressure. The variation in the average maximum lateral pressure with the richness of the mix for different rates of pour was as indicated in Table 6.

TABLE 6

Rate of pour : ft/hr	Mix	Average pressure : lb./sq. ft	Percentage of 1 : 2 : 4-mix pressure
1·8	1 : 1½ : 3	290	100
	1 : 2 : 4	270	93
	1 : 2¼ : 3¾	100	35
	1 : 3 : 6	105	36
3·5	1 : 1½ : 3	350	166
	1 : 2 : 4	210	100
	1 : 2¼ : 3¾	110	52
	1 : 3 : 6	90	43
7·3	1 : 1½ : 3	570	275
	1 : 2 : 4	207	100
	1 : 3 : 6	115	55

The average relation between the mixes is :—

1 : 1½ : 3	.. ..	178 per cent
1 : 2 : 4	.. ..	100 ,,
1 : 2¼ : 3¾	.. ..	43 ,,
1 : 3 : 6	.. ..	45 ,,

The above results show a tendency for the pressures produced by both the lean and rich mixes to increase more rapidly than those for the normal mix, as the rate of pouring increases. However, the wide scattering of the individual results, and the limited data for the 1 : 2¼ : 3¾ mix, which differ considerably from the data for the 1 : 2 : 4 mix, make it difficult to draw any definite conclusions. Smith also found that in general the maximum pressure increased as the consistency of the concrete was made drier, within the limit of workability. This is the opposite of what might be expected. Smith considered that it was probably due to the fact that, under the usual conditions of placing, dry concrete required more tamping

and, because of its dryness, seemed to develop a permanent wedging action between the particles. In the case of wet or sloppy concrete this wedging action does not exist. For low heads the dry concrete (when tamped as

TABLE 7

Rate of pour : feet per hour	Temperature : ° F.	Maximum pressure : lb./sq. ft	Percentage of pressure at 70° F.	
Maxton				
2	80	340	92	
	70	370	100	
	60	400	108	
	50	450	122	
	40	530	143	
3	80	460	94	
	70	490	100	
	60	530	108	
	50	630	128	
	40	730	149	
4	80	540	93	
	70	580	100	
	60	650	112	
	50	720	124	
	40	900	155	
5	80	630	94	
	70	670	100	
	60	750	112	
	50	870	130	Average percentage
	40	1,050	157	
6	80	680	93	93
	70	730	100	100
	60	840	115	111
	50	980	134	128
	40	1,180	162	153
Portland Cement Association				
2	70	350	100	
	50	450	129	
3	70	450	100	
	50	600	133	
4	70	550	100	
	50	750	136	
5	70	650	100	Average percentage
	50	900	138	
6	70	750	100	100
	50	1,050	140	133

usual) gave the greater lateral pressure, but for heads of 4 feet or more sloppy mixes gave the greater pressure.

Temperature of the Concrete

Roby [1] showed by the results of his tests that the pressures developed during hot weather are less than those under moderate temperatures. Mixtures having a placement temperature of 100° F. developed maximum form pressures equal to 60 to 75 per cent of those developed by the same mixes at 60° F.

Teller [11] quotes a case where two columns were filled with vibrated concrete at a temperature of 74° F.; a pressure of approximately 110 per cent of that for a similar concrete mix at 49° F. was developed. However, another variable was present in that the former concrete had a 3½-inch slump whilst the latter had a 1½-inch slump, which might have influenced the relation to some extent.

In the form-pressure curves presented by Maxton [15] (see *Fig. 4*) the effect of temperature of concrete on the concrete pressure has been allowed for. As previously mentioned, these curves are based on data from the U.S. Army Engineers and the Portland Cement Association. The values of the maximum pressure for each curve have been correlated in Table 7, and calculated as a function of the pressure at a temperature of 70° F., which has been taken as a nominal value. The average values show a marked tendency for the pressure to increase as the temperature decreases. There is also a tendency for the maximum pressure to increase relative to the pressure at 70° F. as the rate of pouring increases.

Figs 5 show the results of tests made by the Universal Form Clamp Company [16] on hand-placed concrete. *Fig. 5* (*a*) gives pressures in wall forms when the concrete has a temperature of 70°F., and *Fig. 5* (*b*) gives similar data when the temperature is 50° F. The maximum pressure for each curve is shown in Table 7, and a tendency similar to that shown by Maxton's data is indicated.

Time of Initial and Final Setting of the Cement

In the tests reported by Roby[1] the cement had the characteristics indicated in Table 8.

Table 8

Location of storage of pat	Average temp. : ° F.	Elapsed time of setting : hours	
		Initial	Final
Ice cooler	45	6	11¼
Moist closet	70	3¼	6¼
Room (pats cracked) .	80	2¼	4¼

The test results shown in *Figs 1* and *10* indicated that the time required for maximum pressures to be reached was much shorter than the time of initial set, particularly for the more rapid rates of filling forms. During his tests Roby found that immediately after the point of maximum pressure was reached, the lower panel of the form could be removed, and the concrete was both stable and firm. The shortest time to reach the maximum pressure was ¾ hour.

Size and Shape of Form

Smith [9] made some field tests and measured the lateral pressure of the concrete against the form, in places where the distance between the form walls differed. The results indicated that the maximum pressures increased slightly with the mass of the concrete when it was wet and sloppy. Reinforcing just inside the form tended to decrease the pressure slightly, but Smith considered that this effect should probably be neglected in determining the final pressures for use in design.

The other investigators reported in the Paper generally used only one size and shape of form work, as listed in Table 1.

Theories of Concrete Pressure Against Forms

Theuer [19] in a review of "Pressure of Concrete Against Forms" by R. Hoffman, quotes the following exponential form of expression for the ratio of conjugate stresses as suggested by Hoffman :

$$\lambda = \lambda_0 e^{-\alpha\left(t - \frac{x}{R}\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where λ denotes the coefficient of conjugate stresses at time t, and at height x above the bottom of the box

λ_0 denotes the value of λ at time $t = 0$

P denotes the lateral pressure

α denotes a coefficient of dimension P^{-1}, and a measure of the rate of decrease of s.

R denotes rate of rise of concrete in forms

λ is considered to be dependent on the nature of the aggregate, water content, and density of the mix ; and α, on the nature of the cement and the temperature. Multiplying (1) by the following :

$$P = \gamma(Rt - x)$$

where γ denotes the specific weight of the mix,

Hoffman arrives at the following expression for the lateral pressure :

$$P = \gamma R \lambda_0 \left(t - \frac{x}{R}\right) e^{-\alpha\left(t - \frac{x}{R}\right)} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

with the two constants to be determined by experiment. Taking Hoffmann's observations (based on tests on a form 3·38 feet high and 3·28 by 7·00 feet in cross-section, and placing superimposed loads on the concrete, so as to maintain an average rate of pour equivalent to 1·16 feet per hour), and assuming $\gamma = 150$ lb. per cubic foot, $R = 1{\cdot}16$ feet per hour, and $x = 3{\cdot}38$ feet, the values of $\lambda_0 = 0{\cdot}93$ and $\alpha = 0{\cdot}12$ are obtained. Hoffman obtains limits (corresponding to before and after vibration) of 0·93 and 0·36 for λ_0, and 0·10 for α in both cases. By means of graphs for different locations of x Hoffman shows that all curves are the same except for a time offset or lag, the maximum value for P being obtained when $x = R\left(t - \frac{1}{\alpha}\right)$ and it is independent of both x and t. This maximum is first reached when $x = 0$ at time $t = \frac{1}{\alpha}$.

It should be noted that, in his tests, Hoffman observed that the maximum lateral pressure occurred after the application of the second superimposed loading, 9 hours after the start of the test.

It is not known on what other tests, if any, the above formulae are based, or if superimposed loading is a valid replacement for concrete in a shallow box. It is doubtful whether such a complicated formula for lateral pressure can be used at the present stage, when there is a marked lack of experimental data.

From the results of his tests, Smith [9] proposed the following empirical formula giving the lateral pressures required to design forms :

$$P = H^{0-2} \cdot R^{0-3} + 0{\cdot}12C - 0{\cdot}3S \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where P denotes resultant lateral pressure in lb. per square inch
R ,, rate of fill in feet per hour
H ,, head of concrete fill
C ,, percentage by volume of cement to combined fine and coarse aggregate
S ,, consistency in inches of slump.

For usual conditions H may be taken as not greater than $\frac{1}{2}R$. For ordinary cement in cold weather, or when continuously poured and well agitated, H may be $\frac{3}{4}R$ when the filling is continuous beyond 1 hour. A second pouring on top of concrete that has been in place for 45 minutes or more does not add to the pressures already existing at the bottom of the fill. The value of P obtained by the formula is the lateral pressure against the form at the lowest point of the fill. Since the pressures are not uniform from top to bottom, but, according to Smith, vary approximately as the ordinates of a parabola, the centre of pressure or point of the resultant may be taken at 0·6 of the height of the fill, H, from the top.

On the basis of data from other investigators, together with his own computed pressure values, Macklin [4] produced the curve shown in *Fig. 12*, relating pressure against rate of pour. It is an empirical curve which serves as an envelope for most of the data, and has the following exponential equation :

$$P = 150R^{\left(\frac{1}{2}+\frac{1}{2\sqrt{R}}\right)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where P and R are as above. This formula is proposed for a 1 : 2 : 4 concrete mix, and correction factors are presented whereby the pressure is increased for richer mixes and decreased for leaner mixes. A further modification is that if vibration is used for consolidating the concrete, a value of 300 lb. per square foot should be added to the pressure obtained from equation (4), irrespective of any other factor. No evidence is given to support the values to be used in either of these modifications.

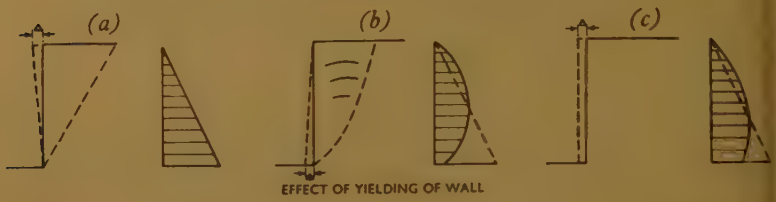
Author's Concept of Concrete Pressure Against Forms

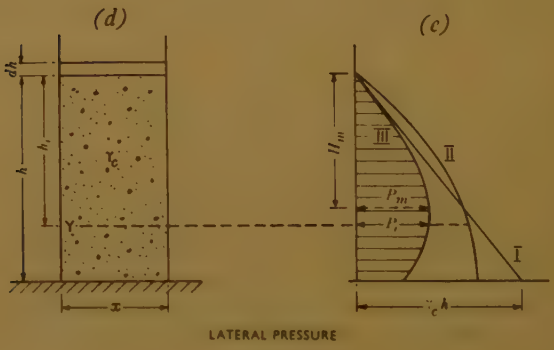
In reading through the numerous publications on concrete pressures against formwork, the Author has noticed an almost completely empirical approach to the problem. This is probably to be expected since the presence of so many variables greatly complicate the problem. There has been very little attempt to give a rational explanation of the variation of lateral pressure against the form, based on the actual physical characteristics of the concrete. The following is an attempt to explain why the pressure distribution is not hydrostatic.

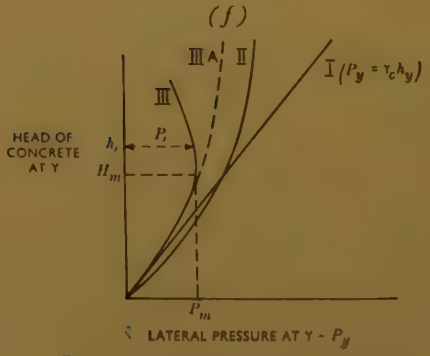
Assuming a particular concrete mix, placed by hand, at a constant temperature and rate of pour, in a form of width x and infinitely long, as shown in *Fig. 11* (*d*). The aggregate settles until the grains are in contact with each other (actually, they are usually separated by a thin film of mortar) and surrounded by the cement-mortar matrix. When the concrete has reached a height h, the lateral pressure distribution should be as shown in curve I, *Fig. 11* (*e*), if the concrete acted as a fluid. It has been shown by Terzaghi [21] that for a granular material, if the lateral support yields by tilting around its lower edge, the pressure distribution is hydrostatic, as shown in *Figs 11* (*a*). However, a yield by tilting around the upper edge is associated with an approximately parabolic pressure distribution (*b*), while a yield parallel to its original position causes a pressure distribution between a parabolic and hydrostatic distribution (*c*). This deviation from the hydrostatic distribution has been explained by Terzaghi as being caused by an " arching effect," resulting from a transfer of pressure from a yielding mass into adjoining stationary parts. Arching also takes place if one part of a yielding support moves out more than the adjoining part.

A study of forms generally used for walls and columns immediately indicates that the type of deflexion that can occur in the formwork is

Figs 11







LATERAL PRESSURE AGAINST FORMS

variable and complex, owing to the presence of the more rigid wales and studs, and the continuously changing moments in these members as the concrete is poured into the form. It is the Author's opinion that some

arching action generally occurs in formwork, and that this effect, in addition to friction between the concrete and form, causes the lateral pressure distribution—neglecting, for the moment, the increasing shearing resistance of the concrete with time—to lay somewhere between cases (*b*) and (*c*), for example as shown by curve II in *Fig. 11* (*e*). The degree of arching action is probably affected primarily by: (1) the yielding and deformations that can occur in the forms, because of the flexibility of the tie rods, wales, studs, and sheathing; and (2) the ratio $\frac{\square}{h}$. The smaller this ratio gets, then, presumably, the greater is the arching effect, since it becomes increasingly difficult for the concrete to deform sufficiently to mobilize its full shearing resistance. The lateral bulging of the sheathing between the stud supports, of the studs between the wale supports, and of the wales between the tie rods, induces concentrations of the pressures in the vicinity of the rigid parts, at the expense of the more yielding parts of the form.

However, the problem is further complicated by the fact that, during the process of pouring, the cement mortar is undergoing a continuous process of hardening, resulting in an increasing shearing strength and rigidity. Hence, it is to be expected that at a point Y, in *Fig. 11* (*d*), as the head h increases, an increase in head, dh, has a decreasing effect on the increase in lateral pressure P, that is to say, as h increases the ratio $\frac{dP}{dh}$ decreases.

Considering the change in lateral pressure at point Y, if the concrete acted as a fluid, the lateral pressure is given by curve I, *Fig. 11* (*f*). Resulting from arching action alone, the pressure increases according to curve II (this was clearly brought out by the measurements on granular material at Barker Dam, see *Fig. 6* (*b*)). Since, however, the shear strength and rigidity of the mortar is continuously increasing, $\frac{dP}{dh}$ decreases with increasing head, and the increase in pressure follows curve III, until a point of maximum lateral pressure, P_m, is reached. The reason for this maximum pressure is not immediately clear, and unless some shrinkage occurred in the concrete at Y, one might expect the pressure to follow curve III A. However, the reason for a decreasing pressure, after the point P_m is reached, might be explained thus:—Up to the time of setting, fresh concrete shrinks and settles in the form with an appreciable decrease in volume owing to expulsion of water or "bleeding". If there is no bleeding or form leakage, the decrease in volume is caused by a portion of the water being absorbed by the aggregate and entering into combination with the cement while the fresh concrete is still in a plastic state. Bleeding and form leakage, however, often contribute more to setting shrinkage than do absorption and chemical change. Initially, then, one would

expect that the head of concrete acting on the section at Y counteracts the shrinkage stresses, and the concrete is in a sufficiently plastic state for it to flow and take up a position of equilibrium with the deflecting formwork, without any appreciable drop in lateral pressure. However, by the time point P_m is reached, owing to the increasing arching action and friction on the side of the forms, the concrete is prevented from settling as much as it would if these restraints were not present, and the shrinkage stresses due to settling become greater than the increase in lateral pressure dP, resulting in a decrease of the total lateral pressure, as h is further increased. This would produce a curve similar to curve III (*Fig. 11* (*f*)), and the distribution of lateral pressure with depth of form would be that given by curve III, *Fig. 11* (*e*).

On this basis, the amount of shrinkage would depend primarily on the loss of water during the filling of the form. It is known that the effect of continuously maintaining an excess of moisture within concrete is, in general, to produce expansion over a long period of time, caused by continued hydration of the cement. Hence, if the concrete at Y is kept continuously saturated, for example as in tremie-placed concrete, the pressure would be expected to follow curve III A. As soon as sufficient pore water has escaped from the pores of the concrete, the colloidal gel in the hardened cement-water paste begins to contract, causing the concrete to shrink, so that the degree of saturation, and the lack or presence of excess water to replace the water used in the hydration process, will probably determine whether the lateral pressure follows curve III or III A. Hence, such factors as the tightness of the formwork and the ability of the material in contact with the concrete to absorb water would affect the lateral pressure of the concrete against the form.

An additional possible cause of a change in lateral pressure at a point might be the effect of the location of the pressure cell. The formwork essentially consists of a series of panels supported on continuous beams acting in two directions, which in turn are supported by a series of point reactions at each corner of the panel. These supports are not rigidly fixed, and they can deflect an amount proportional to the load upon them. The condition of the concrete is continuously changing, being more rigid in the lower part of the pour. As the head of concrete is increased, there is (because of the arching action and stiffening of the lower concrete) a correspondingly larger increase in the lateral pressure of the upper part of the pour, producing different changes in moments at different parts of the form, and the effect on the pressure recorded by a cell will depend on its location. It is conceivable that at point Y a condition might be reached where, with an additional increase in head, dh, the resulting changes in movement might tend to cause the form to bend outwards at Y. The concrete, having stiffened during the period of pour, might be unable to follow this change in the formwork without mobilizing an increasing shearing resistance, and so lowering the actual lateral pressure, until a

position of equilibrium with the formwork is reached. It is also possible that the changing moments resulting from the increasing height of concrete might cause a reverse change at a point such as Y, that is to say, change the pressure from an active to a passive state, and so greatly affect the pressure reading given by the cell, particularly where the latter requires movement against the hardening concrete. It is difficult to picture, qualitatively, the exact effect of these changing moments on the pressure curve, since it will depend upon the design and flexibility of the form, and the location of the cell, as well as the rate of hardening of the concrete.

Analysis of Experimental Data

(1) *Rate of Pour*

(*a*) *Hand-placed concrete.*—In order to correlate the results of these investigations, the available data have been plotted in *Figs 12*, *13*, and *14*. *Fig. 12* shows the relation between the maximum recorded pressure and the rate of pour ; *Fig. 13* shows the relation between the head of concrete at maximum pressure and the rate of pour, and *Fig. 14* shows the effect of the rate of pour on the time required to reach the maximum pressure. In an attempt to cancel out the effects of temperature of the concrete and the type of concrete, where the test results included such data, only those results for nominal 1 : 2 : 4 concrete mixes poured at a temperature of about 70° F. are presented, since these represent average conditions.

The spread of the different test data is considerable, more so than is normally expected from concrete. Assuming that the effects of the variables—temperature and concrete mixes—have been reduced to a negligible factor, which, as a first step, is a reasonable approximation, then the large scattering of the results may be attributed primarily to the following factors :—

(i) Reliability and accuracy of the methods for measuring the concrete pressures, which, as previously explained, vary between different investigations. There is no way of " correcting " the results.

(ii) Variation in size, shape, and flexibility of the form. There is insufficient information to make any allowance either for this factor, or for the relative location of the cell with respect to the more rigid or more flexible parts of the formwork.

(iii) Variations in the consistency and proportions of the mixes used by the different investigators. An attempt has been made, where different mixes were used, to plot only the data for 1 : 2 : 4 mixes, but little could be done to allow for the widely different consistencies that were used.

Since the test results are too few and the scatter too large, it is not possible to obtain an accurate mathematical relationship between the factors plotted. In order to produce a general approximate relationship which may be used for design purposes, the Author has drawn in each diagram a curve which is approximately an envelope to, and which closely follows the trend of, the plotted points. These curves are purely empirical,

and in drawing them, an attempt has been made to account for the probable reliability of the data. The curves may be represented by the following equations :

$$P_m = 400\ R^{\frac{1}{2}} \text{ lb. per square foot} \quad . \quad . \quad . \quad . \quad (5)$$

$$= 110\ H_m \text{ lb. per square foot} \quad . \quad . \quad . \quad . \quad (6)$$

$$H_m = 3{\cdot}6\ R^{\frac{1}{2}} \text{ feet} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$T_m = \frac{3{\cdot}6}{R^{\frac{1}{2}}} \text{ hours} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where P_m denotes maximum concrete pressure, in lb. per square foot
R ,, rate of pouring, in feet per hour
H_m ,, head of concrete at maximum pressure, in feet
T_m ,, time to reach maximum pressure, in hours.

The following observations are of interest :—

(i) The maximum concrete pressure, with respect to the head of concrete at that point, is equivalent to a fluid hydrostatic pressure of 110 lb. per square foot, which is only 75 per cent of the full pressure of a liquid weighing 150 lb. per cubic foot (this value being the average density of concrete). This compares with the values generally quoted in textbooks, that is to say, the lateral pressure should be assumed to be the equivalent of the hydrostatic pressure of a liquid weighing between 125 and 145 lb. per cubic foot.

(ii) The time required to reach the maximum pressure varies from about ½ hour, for rapid rates of pouring, to about 2 hours for slow rates of pouring. The greatest variation occurs at the lower rates of pour.

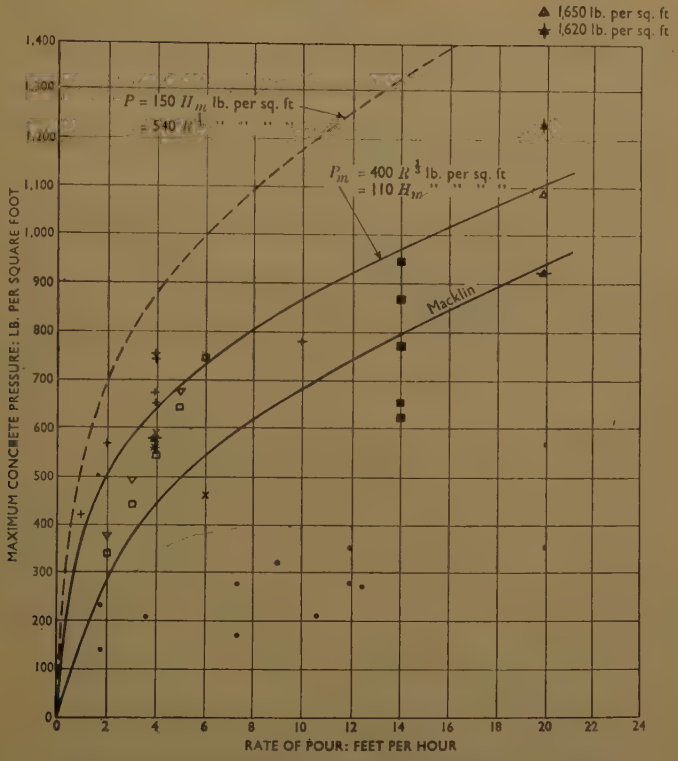
(iii) The head of concrete at which the maximum pressure occurs increases at a decreasing rate as the rate of pouring increases.

This appears to indicate that at low rates of pour the rate of hardening of the concrete has a predominating effect in determining the head at which the maximum pressure occurs. But at high rates of pour it is probably the effect of arching which largely determines the head at maximum pressure; for example, McDaniel and Garver[3] obtained maximum

Symbol	Investigator	Mix	Slump	Placed by
+	Roby	1:2:3½	+ 7" * 3"	Hand
•	Smith	1:2:4	9½"-11" or sloppy	"
×	Slater and Goldbeck	1:2:1½	?	"
△	Teller	1:2:4	△ 7½" ▲ 3½"	"
▽	Maxton	?	?	"
□	Universal Form Clamp Co.	?	?	"
⊙	Bureau of Yards and Docks	7 bags of cement per cubic yard	6½"	Tremie
■	Stanton	?	2"	Vibration
▲	Teller	1:2:4	▲ 1½" ▲ 3½" ✦ 5"	"

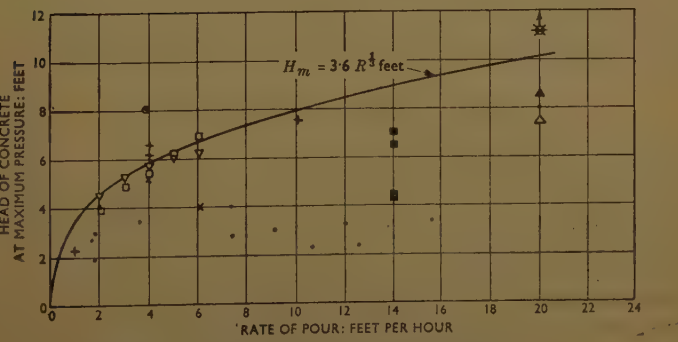
KEY TO *Figs 12, 13, 14, 15, 16,* AND *17*

Fig. 12


▲ 1,650 lb. per sq. ft
★ 1,620 lb. per sq. ft
$P = 150\ H_m$ lb. per sq. ft
$= 540\ R^{\frac{1}{2}}$ " " " "
$P_m = 400\ R^{\frac{1}{2}}$ lb. per sq. ft
$= 110\ H_m$ " " " "
Macklin
MAXIMUM CONCRETE PRESSURE: LB. PER SQUARE FOOT
RATE OF POUR: FEET PER HOUR

Relation between Maximum Pressure and Rate of Pour

Fig. 13



Head of Concrete at Maximum Pressure

pressures at a head of 10 feet, for a rate of pour of about 100 feet per hour; that is, in about 6 minutes, during which only a negligible increase in concrete strength could have occurred. Since for a given form, the arching action should depend little, if at all, on the rate of pour, it would be expected that for high rates of pour there would be little change in the head producing maximum pressure and the value of this pressure, any change being largely due to the hardening of the concrete. This seems to be brought out by the trend of the plotted data.

(*b*) *Vibrated concrete.*—The available test data for vibrated concrete are very scanty. Where external vibrators are used, the full depth of concrete is in agitation, and appears to act as a fluid, so that the forms should be designed for the full hydrostatic pressure of a liquid having the same density as the concrete. This is in accord with the results obtained by Teller,[11] and shown in *Fig. 3.* However, where internal vibrators are used, it is the normal practice to insert the vibrator into the top 2 feet or so of the concrete, and as the height of pour increases, it seems reasonable to assume that only the mass of concrete in the upper few feet will be affected by the vibratory agitation sufficiently to produce a full fluid pressure. Below this depth, it might be expected that the concrete will behave in a manner similar to hand-placed concrete. Further, the narrower the width of the form, the greater is the effect of internal vibration on the stresses in the formwork. The test results obtained by Stanton[5] indicate such an action (see *Fig. 7*) by the fairly sudden departure of the measured pressure from the full hydrostatic curve. Also, Davis and Davis[18] state that maximum form pressures may be expected to be of the order of 450 to 600 lb. per square foot. However, although it would be too conservative to design for the full hydrostatic pressure for the total depth of pour, there is no information regarding the effective depth of agitation for which the full hydrostatic pressure is acting. Hence, the Author considers that, at this stage, it might be satisfactory to assume a behaviour similar to that of hand-placed concrete, in that the concrete pressure increases to a maximum value at a head given by the curve in *Fig. 13* and equation (7), but assuming a full hydrostatic pressure, rather than only 75 per cent. For concrete compacted with an internal vibrator and weighing 150 lb. per cubic foot, this gives a maximum lateral pressure of:

$$P_m = 540\ R^{\frac{1}{2}} \text{ lb. per square foot} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

This curve has been plotted in *Fig. 12.*

(*c*) *Tremie-placed concrete.*—The experimental data are meagre. Both the investigations[7, 8] reported show that the pressures built up to a maximum and then generally dropped off, and that, for the rates of pour of 3 to 4 feet per hour, the maximum pressure was usually reached in $1\frac{1}{2}$ to 3 hours. In the report by the Bureau of Yards and Docks[8] a rather comprehensive series of pressure curves are given. Three typical curves are shown in *Fig. 9.* Most curves show a decline in pressure varying

Fig. 14

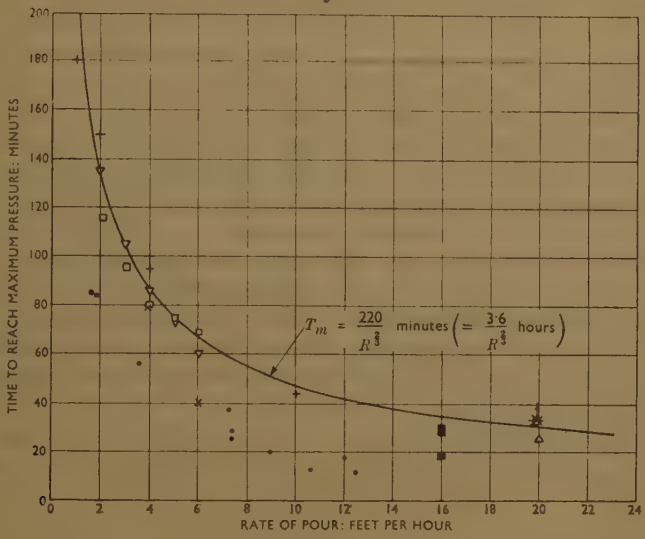
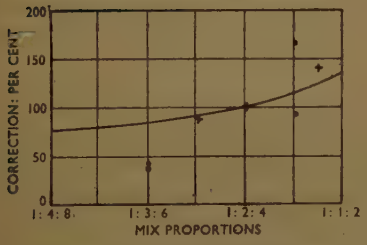
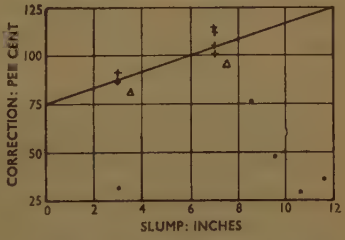

TIME TO REACH MAXIMUM PRESSURE: MINUTES
RATE OF POUR: FEET PER HOUR
$T_m = \frac{220}{R^{\frac{3}{4}}}$ minutes $\left(= \frac{3{\cdot}6}{R^{\frac{3}{4}}}\right.$ hours$\left.\right)$

Fig. 15



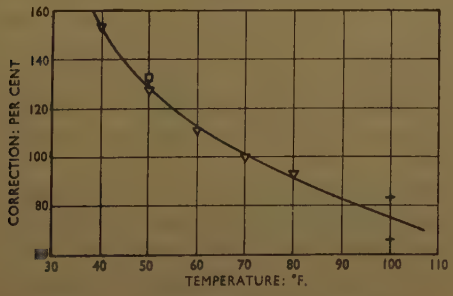
EFFECT OF MIX PROPORTIONS

Fig. 16



EFFECT OF SLUMP OF CONCRETE

Fig. 17



EFFECT OF TEMPERATURE OF CONCRETE

from a small to an appreciable amount, which the authors state was probably the result of the combined effects of shrinkage of concrete and deflexion of the forms owing to loading of the upper portions of the form. It seems doubtful, however, whether any shrinkage occurred in the setting concrete, since it was completely submerged in wood forms. It was also noted in most of the wall-form tests that the decline in pressure intensity ceased, often while the head of concrete was still increasing, and the pressure began to rise again at a continuous rate. This was explained as being caused by the continued hydration of the cement and also by some excess thermal expansion of the concrete compared with the linear expansion of the steel tie rods. It is probable that sufficient time had elapsed for the heat of hydration to raise the initial temperature of the mass of concrete. Although this rise in pressure may be of considerable magnitude, it occurs at sufficient time after initial set to preclude any serious danger from failure.

Since there are no data to indicate the effect of rate of pour on the lateral pressures produced by tremie-placed concrete, no general conclusions can be drawn.

(2) *Consistency and Proportions of Mix*

The only investigators who have published any information on the effect of the mix proportions on hand-placed concrete are Roby [1] and Smith.[9] The results of their tests have been plotted in *Fig. 15*, to show the values of the ratio :

$$\frac{\text{maximum pressure produced by a particular mix}}{\text{maximum pressure produced by 1 : 2 : 4 mix}} \times 100 \text{ per cent}$$

for the different mixes, at the same rate of pour. As indicated by these tests, the maximum pressure increases as the mix becomes richer in cement content. The curve drawn by the Author, to show the correction factors that should be applied to the curves drawn on *Figs 12*, *13*, and *14*, which are for 1 : 2 : 4 mixes, is probably somewhat on the conservative side owing to the scant information available. The correction factor for a very rich mix, that is, a 1 : 1 : 2 mix, has been chosen as 136 per cent, which gives a maximum pressure equal to the full hydrostatic pressure at the depth where the maximum pressure is obtained. For a very lean (1 : 4 : 8) mix, a correction factor of 75 per cent has been tentatively chosen, since it would be expected that the leaner the mix, the greater would be the arching action.

It seems reasonable to expect that the consistency, or water/cement ratio, of the mix has an equally important effect on the lateral pressure, that is to say, for a particular mix, the higher the water/cement ratio the greater would be the equivalent hydrostatic pressure, principally because of a reduction in the strength of the mortar. The only investigators who

used concrete mixes with varying slumps were Roby,[1] Smith,[9] and Teller.[11] In *Fig. 16*, the values of the ratio :

$$\frac{\text{maximum pressure produced by a particular slump}}{\text{theoretical maximum pressure for same rate of pour}} \times 100 \text{ per cent}$$

have been plotted against the slump of the mix. The theoretical maximum pressure is that given by the curve $P_m = 400\,R^{\frac{1}{2}}$ in *Fig. 12*, and corrected for the particular mix by means of the curve in *Fig. 15*. The data from Roby and Teller, which appear more consistent than the data from Smith, show a definite increase in pressure as the slump increases and becomes more " fluid," and the curve has been drawn to attempt to show the effect of consistency. It is fully realized that this curve is of a very approximate nature, owing to the small amount of experimental data. It is possible that the drier and lower-slump concrete, requiring more tamping and greater effort to compact the material in place, might tend to increase the lateral pressure, and so counterbalance the effect of slump as shown by the curve in *Fig. 16*.

(3) *Temperature of Concrete*

The design curves published by Maxton[15] and the Portland Cement Association[16] both allow for the effect of temperature on form pressures. The values of the ratio :

$$\frac{\text{maximum pressure for a particular temperature}}{\text{maximum pressure for 70° F.}} \times 100 \text{ per cent}$$

have been plotted against temperature in *Fig. 17*. The values obtained by Roby[1] have also been plotted. An average curve has been drawn to show the relation between the temperature correction, to be applied to the curve $P_m = 400\,R^{\frac{1}{2}}$ in *Fig. 12*, and the temperature of the concrete at the time of placing. It shows an appreciable rise in maximum pressure as the temperature decreases, which is probably caused by the lower rate of hardening of the cement. It should be pointed out that the methods used in the tests, from which the above data[15,16] were obtained, have not been published—which leaves some doubt as to the reliability of the curve in *Fig. 17*.

(4) *Time of Setting of the Cement*

As shown in *Fig. 14*, the time required to reach the maximum pressure ranges from between 2 and 3 hours for very slow rates of pour to less than 20 minutes for the rapid rates of pour. Hence, there is no direct relation between the maximum lateral pressure and the time of " initial " and " final " setting of the cement. The Author prefers to think of this factor in terms of the rate of increase in shearing resistance of the cement mortar with time, rather than the quantity " time of initial setting." It is the shearing strength of the mortar which has a direct relation to lateral

pressure, and which comes into action as soon as the concrete is placed. Since there are no experimental data for tests with cements having different rates of hardening, it is not possible to determine any such relation.

(5) *Size and Shape of Form*

There are hardly any experimental data on the effect of the size and shape of the form on the lateral pressure from which any conclusions can be drawn, other than that the data appear to exhibit a tendency for the maximum pressures to be smaller in the forms with smaller cross-sections. If, as has been previously stated, arching occurs and has an important effect on the lateral pressure, then it is to be expected that the smaller the width of the formwork, the greater would be the arching action. Another important item is the general flexibility of the form. However, there are no data either on this problem, or on the effect of the presence and amount of reinforcing, which latter might be expected to carry part of the concrete load and so have some beneficial effect on form pressure, although this might be cancelled by increased tamping necessary to distribute the concrete between the reinforcements.

(6) *Decrease in Lateral Pressure after Maximum Pressure is Reached*

In much of the test data described in this Paper, pressure measurements ceased shortly after the maximum pressure had been obtained. The remaining data indicated that the measured pressure decreased very slowly or at a rate almost as great as the rate at which the pressure built up to a maximum. The charts published by the Portland Cement Association (see *Figs 5*) show that the pressure drops to zero 3 hours after the maximum pressure is reached, this time being independent of the rate of pour and temperature.

For continuous shuttering, Reynolds[20] states that, "The rate of vertical progress may be 10 feet per 24 hours. In favourable conditions an average of nearly 1 foot per hour has been achieved." Hence, the value of 3 hours required for the pressure to decrease from its maximum value to zero seems to be a reasonable figure for use in normal form design.

Shape of Pressure Diagram for Design of Forms

From the foregoing data, given the rate of pour, mix proportions, slump of concrete, and temperature at the time of placing, the maximum pressure P_m, and the head of concrete at maximum pressure H_m, can be estimated, for hand-placed concrete. Although the actual pressure distribution against the form is likely to be similar to curve B in *Figs 18*, for the purpose of design it is simpler to assume a linear distribution between zero and P_m, that is, curve A. The error in this assumption is small, and partially cancelled out by the fact that, for the sake of economy, the normal practice is to use the same size sheathing throughout the depth

of pour, the sheathing being designed to withstand the maximum pressure P_m. As the head of concrete increases, the value of $(H-H_m)$ increases, H_m and P_m remaining constant, thus causing changes in moments in the studs and wales, which should, therefore, be designed for the highest moments obtained during the pour. The same procedure applies to the tie rods, which usually are also made a constant size.

The effects of the impact produced by the instantaneous dumping of buckets or the height of fall of the concrete are not taken into account. Generally, the local impact loading will be absorbed by the factor of safety of 4, or more, which is usually recommended for design stresses, and need not be considered for theoretical deflexion of the members.

Conclusions

Owing to the large number of variables affecting the lateral pressure of concrete against forms, nearly all investigators have adopted a completely empirical approach to the problem. In many cases, the investigators have merely attempted to find the lateral pressure for particular construction conditions, rather than investigate the general effect of the different variables. Much of the data are comparatively old, particularly with regard to the different types of pressure measuring devices, which throws considerable doubt on the reliability and accuracy of the results obtained, and it is not possible to correct, quantitatively, for their degree of reliability.

The Author has attempted to give a rational explanation of the physical phenomena causing the types of pressure distribution found in practice, and to explain why this is not hydrostatic, except in special circumstances. For particular concrete conditions, the Author believes that the following physical factors determine the general shape of the pressure distribution against the form :

(*a*) the degree of the arching action of the aggregate ;
(*b*) the rate of hardening of the cement mortar ;
(*c*) the setting shrinkage of the cement ;
(*d*) the relative rigidity of the formwork, and the changing moments, as the head of concrete is increased ;
(*e*) the method of placing the concrete.

The greater the degree of the factors (*a*) to (*d*) the more will the pressure distribution tend to deviate from the equivalent hydrostatic pressure of a fluid having the same density as the concrete. For hand-placed and internally vibrated concrete, these factors usually result in a lateral pressure distribution similar to curve III in *Fig. 11* (*e*), with a maximum lateral pressure P_m occurring at a depth H_m below the surface of the pour, and the value of P_m being greater for the internally vibrated concrete.

For concrete vibrated externally, the pressure distribution is hydrostatic as shown by curve I in *Fig. 11* (*e*).

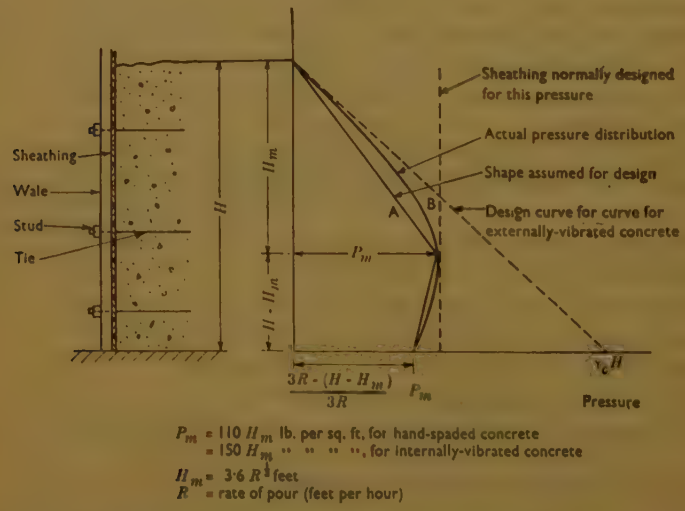
The principle factors and concrete characteristics affecting the values of P_m and H_m are :—

(*a*) *Rate of filling the forms.*—As the rate of pour increases both P_m and H_m increase at a decreasing rate, indicating that at low rates of pour the hardening of the cement has a more important effect than the arching action, and conversely at high rates of pour. The time required to reach P_m ranges from about 2 hours for rates of pour of 2 to 3 feet per hour to less than 30 minutes for rates of pour of more than 20 feet per hour. From the available test data, the following general equations should prove satisfactory for designing forms for a 1 : 2 : 4 mix, at a temperature of about 70° F. :

$P_m = 110\ H_m$ lb. per square foot, for hand-placed concrete
$= 150\ H_m$ lb. per square foot, for internally vibrated concrete
$H_m = 3{\cdot}6\ R^{\frac{1}{2}}$ ft, where R denotes rate of pour, feet per hour.

(*b*) *Proportions of the concrete mix.*—The small amount of data indicates that the richer the mix the greater is the value of P_m, possibly because of

Figs 18



SHAPE OF PRESSURE DIAGRAM FOR DESIGN OF FORMS

the increasing lubricating action of the cement and hence a decreasing arching action, and also an increasing strength.

(*c*) *Consistency of the concrete.*—The Author would expect that, for a particular mix, the higher the water/cement ratio, or slump, the smaller would be the deviation of the pressure from a hydrostatic distribution,

resulting from a reduction in the strength of the mortar and causing the concrete to be more "fluid." There are very little experimental data for this factor, but what do exist tend to confirm this conclusion.

(*d*) *Temperature of the concrete.*—The limited amount of data shows an appreciable rise in P_m with a decrease in temperature, and this is probably because of the lower rate of hardening of the cement.

(*e*) *Size and shape of the form.*—There are hardly any experimental data to determine the effect of this factor. Generally, the Author considers that the smaller the width of the form, the greater would be the arching action. Also the presence of reinforcing steel might tend to reduce the lateral pressure.

For the design of forms, the Author suggests the use of the pressure diagram shown by curve A, in *Figs 18*, which indicates pressures a little lower than those normally recommended in textbooks. However, there is a lack of reliable experimental data on the effect of each of the above factors, and an extensive and well-planned study, particularly experimental, is essential to obtain a rational picture of the lateral pressure distribution of concrete against forms. Until this is achieved, the design of forms has, of necessity, to be rather conservative—representing, in many cases, an unnecessary increase in the cost of construction.

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The Paper is accompanied by twelve sheets of diagrams from which the Figures in the text have been prepared.

CORRESPONDENCE

on Papers published in Proceedings Part I, March 1952

Paper No. 5834

"A Laboratory Investigation of Some Bridge-Deck Systems" †

by

Frederick George Thomas, Ph.D., B.Sc., M.I.C.E., and Andrew Short, M.Sc.

Correspondence

Mr Samuel McConnel, of Nairobi, Kenya, observed that, outside the British Isles, filler-joist and jack-arch construction were of little interest, and in view of the considerable unnecessary weight and waste of materials, those types could almost be termed barbaric. It was nearly 40 years since he had had anything to do with them.

Incidentally, bridges of the filler-joist type had been used with spans of up to 70 feet on the Continent, using rolled-steel joists 1 metre in depth. An example existed in Basle.

In colonial practice, simple concrete slabs were used for spans of up to about 20 feet. Many hundreds of miles of bridges with spans of 20 to 40 feet had been built with steel or reinforced-concrete beams. Where skilled labour was difficult to obtain, rolled-steel joists had been used at centres ranging from 5 to 7 feet, on which a concrete slab was supported. That type was particularly useful in view of its ease of erection, since the shuttering was carried by the R.S.Js. About 25 years ago it had been realized that if proper shear connectors were provided between the joint and the concrete, the design could be made to conform to the actual conditions and very considerable economy would ensue. Probably the best type of shear connector consisted of small lengths of channel section welded to the R.S.J. flanges at suitable intervals.

Where skilled supervision was possible, reinforced-concrete beams should be substituted for the R.S.Js, which would lead to considerable saving in the weight of steel—now almost a precious metal—and in multi-span bridges advantage could be taken of continuity.

Mr McConnel drew attention to the "Standard Specifications for Highway Bridges,"[1] which was of great value to all concerned with bridge

† Proc. Instn Civ. Engrs, Part I, vol. I, p.125 (Mar. 1952).

[1] "Standard Specifications for Highway Bridges." A.A.S.H.O., Washington, 1944.

construction. Innumerable experiments had been carried out in the United States on bridges of the normal type, and even if no shear connectors were used between concrete slab and the R.S.Js, the friction and adhesion on the abutting surface had been found to transmit a considerable amount of shear.

To his knowledge, at least three bridges of that type had been constructed in East Africa. The R.S.Js were, according to standard methods of design, stressed up to the elastic limit by the dead load alone. He also knew of another case in which the same conditions applied to the steel beams in an important building.

It was not to be assumed that such methods were justifiable, but undoubtedly many structures depended for their strength on factors which their designers had ignored.

There was often a tendency to design for a combination of the most improbable conditions, and to be averse to allowing anything to be covered by the factor of safety.

As Sir Benjamin Baker had once stated at the Institution, " Designers often make assumptions in design which are more convenient than accurate."

Considerable economy was possible in the design of abutments for spans of up to 40 feet by designing them as vertical beams strutted by the superstructure.

The Authors, in reply, stated that the information on colonial bridge-building practice contributed by Mr McConnel was of great interest. With regard to slab-and-girder systems, the inclusion of properly designed shear connectors would greatly increase the load-bearing capacity of most bridges of that type, although it seemed doubtful whether design based on the elastic theory would invariably take full advantage of that increase. Opinions also diverged as to the choice of the most suitable shear connector. Further economy in the use of steel might be achieved by replacing steel girders by pre-stressed concrete joists in composite structures.

There was no doubt, so far as modern British design practice was concerned, that forces caused solely by friction due to selfweight, between adjoining surfaces of the components of slab-and-girder systems, ought not to be taken into account when estimating the degree of composite action between them. The experiments conducted at the Building Research Station on large-scale models supported that view. There was in any case considerable room for rational economy by including in the design other factors mentioned in the Paper and—in a more advanced stage of development—by basing the design of composite structures on their ultimate load-bearing capacity rather than on the perhaps somewhat arbitrary criterion of permissible stress coupled with the elastic theory, as at present.

Paper No. 5848

"A New Method of Gauging Stream Flow with Little Afflux by Means of a Submerged Weir of Triangular Profile" †

by

Edwin Samuel Crump, C.I.E.

Correspondence

Mr Gerald Lacey observed that the Author's novel method of gauging stream flow would appear to hold out most promise when the afflux was small and the range of discharge not too great. It would lend itself, for example, to the measurement of a moderate range of discharges on a canal where every loss of head had to be reduced to the minimum. The crest of the weir could be combined with the piers of a road or railway bridge.

The Author disregarded the results for the two lower discharges of 0·577 and 0·793 cusec respectively on the grounds of "scale effect," but it was possible that that persisted, to a smaller extent, even with the larger discharges. A study of *Fig. 4* showed that the raising of the bed upstream by means of gravel had a very marked effect on the values of the coefficient C_T. The data given in Table 6 were for a limited range of the ratio P/T. Instead of drawing a smooth curve through the numerous results of Table 4, Mr Lacey had attempted a logarithmic correlation by treating Tables 3 and 4 as one collection of data. It should be noted that Table 3 was the most complete collection of observations.

In *Fig. 6*, he had plotted values of $(1 - P/T)$ against values of C_T for Tables 3 and 4. It would be seen that there were two distinct stages of flow and that two equations were necessary. For values of $(1 - P/T)$ of 0·10 and less the equation was:

$$C_T = 6{\cdot}85\ (1 - P/T)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and for values of $(1 - P/T)$ exceeding 0·10:

$$C_T = 3{\cdot}85\ (1 - P/T)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The fit was quite good and the equations could easily be solved using a slide-rule.

The afflux was a minimum when $(1 - P/T)$ was minimum. For that condition the discharge in unit width q was equal to:

$$6{\cdot}85\ (1 - P/T)^{\frac{1}{2}} T^{\frac{3}{2}} = 6{\cdot}85\ \sqrt{(T - P)}\ T$$

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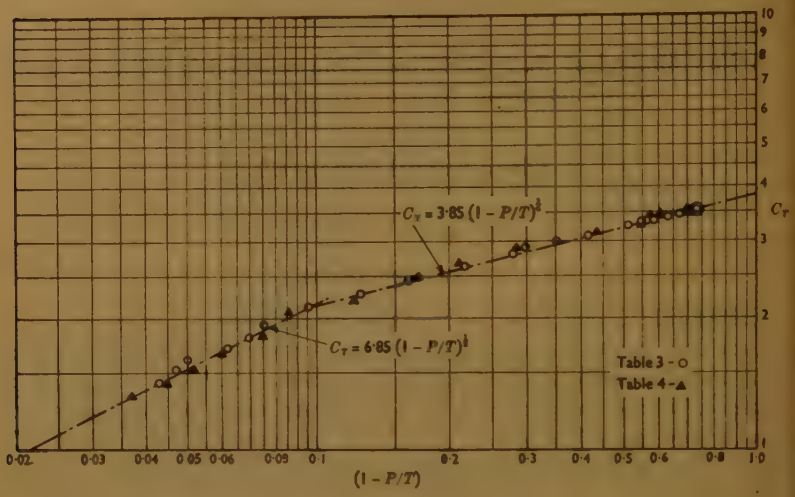
† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 223 (Mar. 1952).

Fig. 6



At the limit, $(T - P)$ was equal to the velocity head, and T was equal to the depth of water. There was therefore a physical explanation for that part of the curve, $(2g)^{\frac{1}{2}}$ being replaced by 6·85.

The limit for P/T would appear to be approximately 0·25, corresponding, in terms of equation (11), to a value of C_T of 3·583.

Mr R. F. Wileman stated that his observations on the Author's valuable contribution towards the solution of the varied problem of satisfactory gauging of rivers, now a statutory duty of River Boards in Great Britain, related to certain aspects of the practical application of the suggested method of double gauging. That duty was a new one, and the advantages resulting from its performance might not at first be so apparent to some as the small inconveniences arising from its combination with the primary duty of drainage-cum-conservation of water in British rivers. It was therefore important to try to reconcile the requirements of the two duties.

Because the tendency, particularly on small rivers, was to avoid so far as possible any pier in the channel, it seemed to Mr Wileman that the Pitot-pier might arouse objection. Even if it were of small and streamlined transverse section—as in the case in question—obstruction might be caused by the lodgement of long pieces of debris between the pier and the channel side.

The small orifices in the Pitot-pier appeared liable to blockage at low stages of flow by floating or partly submerged vegetation. Might that not interfere with the Pitot gauging until it was seen and remedied ? With regard to the piezometric holes in the crest duct, would the Author please

say if the conditions of flow over the crest would be certain to prevent gradual blockage of the duct. It seemed to Mr Wileman, however, that even if solid matter did gradually accumulate in the duct, clearance might be easily effected by means of a pump at low stages.

It would be of great interest to know the opinion of instrument makers concerning the production and possible cost of an instrument which could be relied upon accurately to record the small difference between Pitot and piezometric readings, upon which the Author's method depended under non-modular conditions of flow. Such an instrument should be comparable, with regard to mechanical reliability, with a good float-operated level-recorder, which, properly installed, was remarkably free from failure.

In conclusion, Mr Wileman referred to an aspect in which the Paper seemed to offer very useful assistance in gauging only under modular conditions of flow, particularly in smaller channels. The Author had calibrated a weir of definite triangular profile, the modular discharge over which was given by formula (9). A weir of such profile was of constant coefficient and not appreciably affected by deposition of silt against its upstream face. Constructed to the proportions of the model, with metal crest, arranged in conjunction with a float-operated discharge-recorder, it should afford ample gauging accuracy under modular flow. In view of the necessity for the minimum lengths of side walls, it appeared that such a weir was not suitable for " compounding " and therefore its use had to be confined to circumstances ensuring that its width would not be excessive in relation to minimum discharge to be measured. Mr Wileman suggested that a low weir of that type, with wing walls of height just sufficient for the purpose, could in many instances be used for measuring the lower stages of discharge occurring during the greater part of a year on small rivers, the higher stages being " covered " by current-meter rating of the channel at an adjacent site. The weir structure, completely submerged at the higher stages, should offer very little obstruction to flood flow provided that its upper limit of gauging capacity was restricted to the lower limit of current-meter rating of the channel. Some extra channel protection would be required. One float-operated instrument, recording levels (only) in its upper range and discharge in the low range, would suffice.

Mr J. W. Horner found it rather disconcerting to find the well-known standing-wave flume described as a " long-throated weir," the Venturi flume as a " non-modular long-throated weir," and a recording instrument measuring the flow through a Venturi flume as a " Burkitt's headless meter."

The Author had confirmed that the standing-wave flume provided the most accurate method for measuring streams and rivers with a minimum loss of head, and that with care in design the flume should not be drowned at any rate of flow. It had frequently been found that a flume having a throat of trapezoidal section was the best for providing minimum loss of

head at all flows, and quite a number of those were now in use. They were, however, rather expensive to construct, and as an alternative, where the flow could vary very widely, standing-wave flumes of a compound type were being used.

In cases where a standing-wave flume became non-modular (that was to say, a Venturi flume) it was usual to determine the flow by measuring the head in the throat as well as upstream of the flume. Mr Horner had never heard of anyone attempting to determine the flow by taking the second measurement downstream of the flume—which the Author seemed to suggest was the usual practice.

Mr Horner gathered from the Paper that the chief aim of the Author had been to produce a design of weir which would have a coefficient of discharge substantially constant under modular flow conditions, and which could be used satisfactorily for measurement purposes both in the modular and non-modular conditions. The design suggested would appear to fulfil those conditions within limits, but Mr Horner hardly thought that the Author could justify all his claims from the test figures produced.

The rates of flow used in the tests had only a small range, namely, from 0·5769 to 1·510 cusec—rather less than 3 to 1—and yet when referring to the curve in *Fig. 4* the Author had rather lightly asked that the figures obtained from the tests shown in Tables 1 and 2, when the flow represented 38 per cent and 52½ per cent of the maximum flow, should be disregarded. Surely those figures could not be disregarded on the ground that "the scale-effect to be expected with a small model is apparent." The figures actually showed the effect of "frictional" losses which reduced the coefficient of discharge on small flows.

Mr Horner also wondered why the Author had only taken figures from Tables 4 and 5 to determine $(C_H')_M$ as 3·456, and had then claimed that coefficient to be correct subject to a tolerance of 1 per cent. The values of $(C_H')_M$ taken from all the Tables seemed to show a value of 3·307 at 0·5769 cusec rising to 3·67 at 1·510 cusec—an increase of 10 per cent in a range of less than 3 to 1.

On most British rivers the range of flow between maximum and minimum was rarely less than 100 to 1, and usually nearer 1,000 to 1, and if all flows were to be measured the weir had to be of a size suitable for measuring the largest flows. On such a weir the "frictional" effect on small flows would be considerable and a coefficient of discharge which might appear to be constant on large flows would be very far from constant on small flows. The same effect was evident on standing-wave flumes when the coefficient 3·09 on a rectangular flume fell considerably on flows which were only a small percentage of the maximum. It seemed to Mr Horner that the same effect was evident in the figures presented by the Author, even though the range of flow was only 3 to 1.

With regard to the practical aspects of the weir, Mr Horner was of the opinion that the cost of maintenance would be heavy on many rivers.

All designs of weirs and standing-wave flumes should be such that the effect of sand, boulders, leaves, and debris was reduced to the minimum, but even so it was no uncommon thing for large connecting pipes and float wells to be choked with sand and debris, and for boulders and stones to be piled up against a weir. He felt therefore that in the Author's weir the small holes leading to the ducts, and the ducts themselves, would very rapidly become choked with silt or leaves and become ineffective. The Pitot orifice in the piers too would become choked, and debris would cling to the piers themselves. No doubt the Author had given that matter some consideration and would have his remedy.

Mr E. Gresty observed that the Author had attempted to develop a new measuring device by a study of known results and a careful selection of features from various devices not normally associated together. The Paper gave a clear account of the successive steps of the reasoning, though the reader had first to master a terminology which saddled well-known appliances with unfamiliar and unattractive names.

An engineer considering a measuring installation would naturally wish to compare the old and new methods. Table 7 was an attempt to compare the new weir with the " standing-wave flume " for a particular case.

The standing-wave flume gave a very high accuracy of measurement over a wide range of flow, provided that a loss of head of 6 or 7 inches could be maintained.

The new form of weir assumed a value of $(C_H')_M$ which remained constant at all rates of flow, and although the Author had stated that the value was 3·456 within 1 per cent, his justification was not very convincing. In addition, for the non-modular range the rate of flow could only be deduced from the *difference* of two gauge readings.

If, at the maximum rate of flow, the difference between upstream and downstream water level was less than about 6 inches, the standing-wave flume became drowned, and the Crump weir offered the advantage of measurement under otherwise almost impossible conditions. The width of weir had, however, to be increased to pass the required flow; for example, a width of 33 feet would be required for a 3-foot loss of head; that would probably lead to non-modular conditions at comparatively low flows. It also involved expensive constructional work and a large number of the special crest sections.

Measurement Officers would feel indebted to the Author for a new approach to the principle of weir flow, and for justifying the popularity of the standing-wave flume, rather than for the introduction of a new method of measurement for widespread application.

Mr A. B. Tiffen agreed that the triangular weir could be a very useful instrument for gauging the flow of streams, and considered the fact that an accumulation of gravel on the upstream side did not affect the measurement of the discharge to be a valuable property, but he thought that there were points that required consideration.

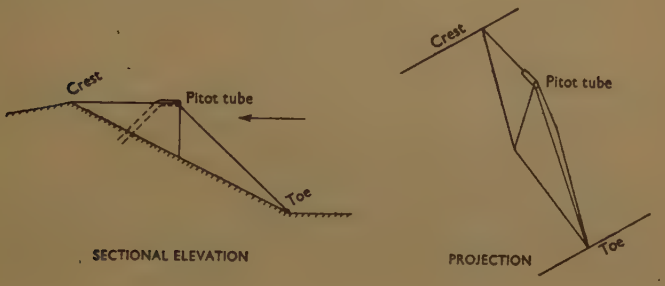
TABLE 7.—COMPARISON OF "STANDING-WAVE FLUME" AND CRUMP WEIR

Maximum rate of flow: 1,000 cusecs. Depth of channel: 10′-0″

	Standing-wave flume. Side contractions	Standing-wave flume. Hump only	Crump weir. (Modular)	Crump weir. (Non-modular)
Height of sill above bed	—	2′ 0″	2′ 6″	28′ 6″
Head above sill	10′ 0″	8′ 0″	7′ 6″	7′ 6″
Width	10′ 0″	14′ 0″	14′ 0″	16′ 6″ 33′ 0″
Width of channel	about 20′ 0″	14′ 0″	14′ 0″	16′ 6″ 33′ 0″
Minimum loss of head between upstream and downstream	6% = 7¼″	6% = 5¾″	30% = 27″	6″ 3″
Accuracy	Very high providing minimum head loss is available			Assumes constant value of $(C_H')_M$. Also depends on difference between two readings
Flooding	Flooding caused upstream if head loss does not exceed the minimum value stated			
Stream velocity	Can be controlled by suitable design		Height of sill causes low velocity at low flows, probably leading to silting	
Lowest flow measurable	Very small—about 3 cusecs with compound instrument, or 50 cusecs with simple instrument		?	?
Type of instrument	Normal type of flow recorder (single float) giving continuous records in rates of flow. Integration if desired		Independent level recorders. "Spot" values of rate of flow can be obtained by reference to curves or tables	
Cost of construction	Rather more extensive concrete work than for weir, but only one float chamber, and normal standard of workmanship satisfactory		In addition to construction of weir, accurate gun-metal crest pieces required, and these must be laid to correct angle. Two float chambers and centre piers	

The Author had not explained why he considered Pitot-piers more practical than tubes. In many streams it was probable that pieces of cloth, clumps of grass, or other trash would be carried along near the bottom and would wrap round the nose of the piers and interfere with the operation of the Pitot-tubes in the piers; and at low flows floating or partly submerged trash would do likewise. To overcome that, tubes could be used without piers but with sloping fins in front which would tend to cause submerged trash to rise and run past the mouths of the tubes. Such an arrangement was illustrated in *Figs 7*. The edges of the fins would, of course, be rounded.

Figs 7



There was no indication of the minimum head at which the weir would function, and since in many cases it was necessary to measure both minimum and maximum discharges, the value of the weir would be limited unless it could be used to measure the former. It appeared that the minimum head might be about 3 inches.

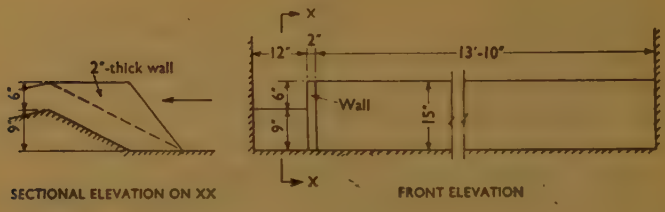
Mr Tiffen considered the problem of an earth channel of 15 feet bottom width which had a maximum flow of about 600 cusecs when running 5 feet deep and a minimum flow of about $\frac{1}{2}$ cusec. A weir 15 feet long with a level crest and with 3 inches head would discharge about $6\frac{1}{2}$ cusecs when modular, and with less head the discharge would be very uncertain, so to solve that difficulty the following method could be used for a practical solution of the problem.

The desirable height of a weir for a depth of water of 5 feet would be 15 inches.

If a 12-inch length at one end of the weir were made 9 inches high, as in *Figs 8*, then with a depth of $3\frac{1}{3}$ inches over the crest of that reduced weir the flow would be about $\frac{1}{2}$ cusec, and low flow could be measured between depths of $3\frac{1}{3}$ inches and 6 inches, corresponding to discharges of $\frac{1}{2}$ and $1\frac{1}{2}$ cusec.

At maximum flow with a 5-foot depth of water, the weir would function much as a 15-inch-high weir right across the full 15-foot width, and the error arising from the dip at one end would probably not exceed 1 per

Figs 8



cent. The discharge would thus be within 1 per cent of that indicated by applying readings obtained from the weir 15 inches high by 13 feet 10 inches long to a weir 15 feet long, whether in the modular or in the non-modular range.

Separate recording apparatus would be required for the separate lengths, but that would not increase the cost of the installation unduly, since at low discharge the flow over the weir would be modular and only the "*T*" reading would need to be recorded.

At intermediate flows, particularly with a small head over the crest of the longer length, the discharge would be somewhat indeterminate, but with those flows great accuracy in measurement would not be necessary and a reasonable estimate could easily be made and a discharge curve plotted.

Mr Tiffen did not quite understand the Author's remark about the scale effect at the two lower discharges in the experiments, and would like that matter further elucidated. Since the velocity over the crest would have been considerably higher than 1 foot per second at modular flow in both cases, Mr Tiffen would have expected no modification of the flow pattern.

Dr M. M. Kansoh, of Cairo, observed that the Author had laid stress upon the "complexities and liability to error" of double-gauging. In that case it was necessary to try to improve single-gauge flow meters by:—

(1) Improving the accuracy by basing the functioning of the device on a sound mathematical foundation.
(2) Widening the modular range.
(3) Reducing the head loss in the device, that was to say, the afflux and dissipation in standing waves.
(4) Reducing the cost by using short inexpensive devices.

On p. 227 of the Paper, Q was considered to be a function of the radius of curvature of the underside of the nappe. In fact, Q was a function of the actual pressure at the section under consideration. The pressure was affected by the upward acceleration due to curvature. The acceleration (or more precisely, the effective acceleration) was dependent not only upon the radius of the underside of the nappe, but also upon some other mean effective radius.

It was stated on p. 229 that, since H' and h' involved Q, the latter could only be calculated by means of a tedious trial-and-error procedure. That could easily be avoided and the discharge calculated directly by applying the formula :

$$Q = C \,.\, a \sqrt{2g(H - h) + V^2} = C \,.\, a \sqrt{2g(H - h) \times \frac{1}{1 - \left(\frac{a}{A}\right)^2}}$$

Dr Kansoh did not see any need, from the point of view of measuring the discharge—especially in the modular range—for the downstream gauge, at which the depth sometimes attained different values for the same discharge and same upstream depth.

A downstream gauge was needed only if it was required to know the submergence or recovery or the total loss in the meter. It was better, therefore, to follow the advice of the Author (p. 232) by constructing a gauge well, especially in the long-throated measuring weirs, at the " control section " at mid-point of the throat, to take the place of the usual downstream gauging station, and thus render the latter unnecessary.

The decision that " in a long-throated measuring weir, the position of the control section is approximately half-way down the throat " was a trifle optimistic. The control section was supposed to be the section where the depth, in the modular range, was critical—that was to say, equal to $\frac{2}{3}H'$ or $\frac{2}{3}T$. Within and beyond the modular range, the value of the depth at the control section was always added to the value of the velocity head at the same section and equated to the total head H' or T. That was equivalent to saying that the depth was equal to the potential head, or to the pressure head plus the position head with the crest level as datum. That was never precisely true except when the flow at the control section was uniform or parallel, with no curvature, convergence, or divergence.

In the hope of finding one definite fixed section fulfilling those two conditions at different rates of flow, Dr Kansoh had experimented on a number of Venturi flumes with throats of different length-to-width ratios and different cross-sectional forms, at different rates of flow, both in the modular and drowned ranges.

As could be observed from the results recorded in *Figs 9* to *14*, no such definite control section could be fixed.

That agreed with Woodburn's [1] and Webb's [2] experiments on weirs of different profiles, as was shown by *Figs 15* and *16*. The small circles denoted what they had believed to be the critical depths.

Those who were especially interested in that point could refer to the work of Bakhmeteff,[3] Rouse,[4] Hinds,[5] and Stevens.[6]

Dr Kansoh also wished to know on what basis, if any, had the Author

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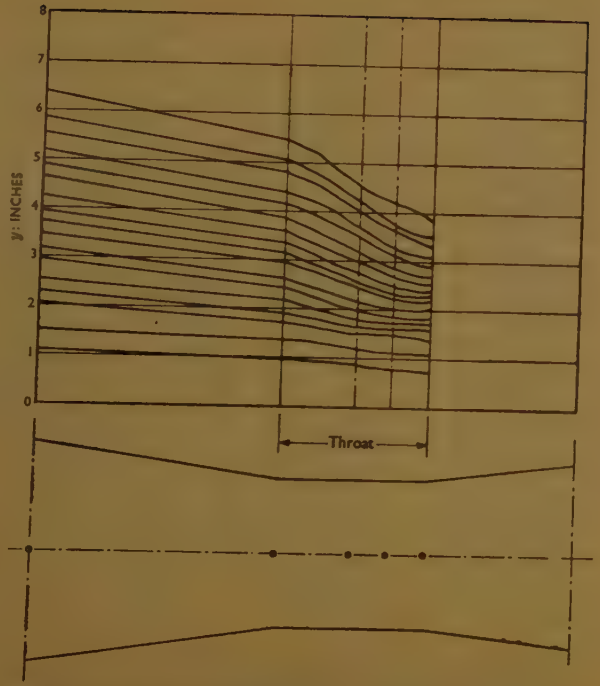
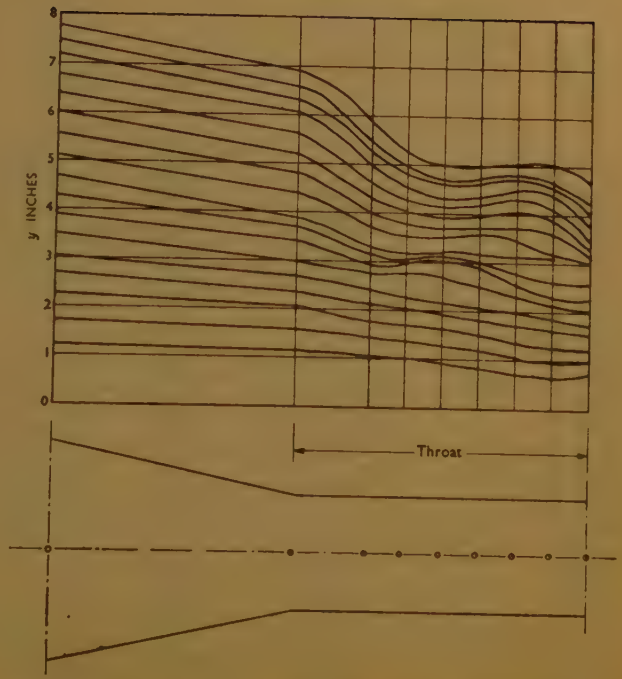
[1] The references are given on p. 767.

Figs 9

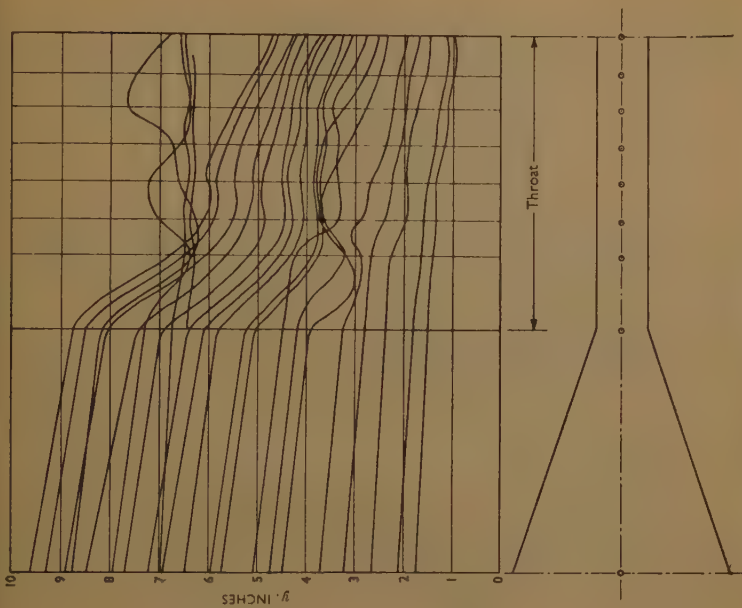


WATER PROFILES IN A RECTANGULAR THROAT, 12 INCHES BY 12 INCHES

Figs 10

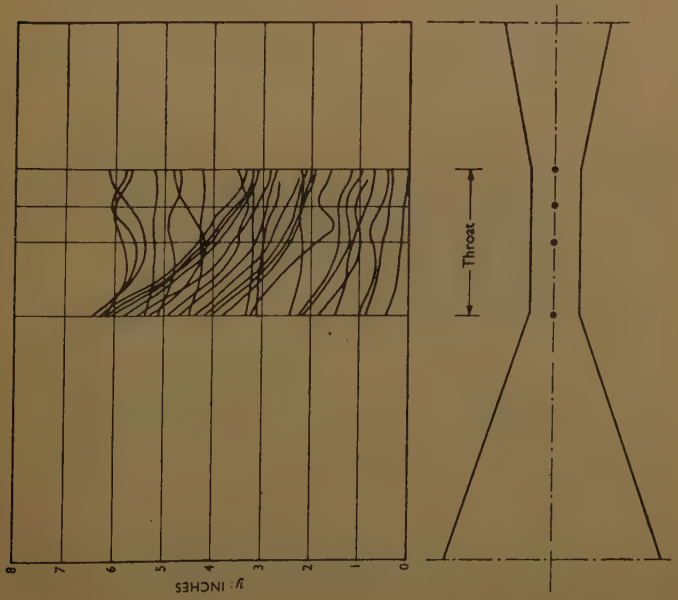


WATER PROFILES IN A RECTANGULAR THROAT, 9 INCHES BY 24 INCHES



Figs 12

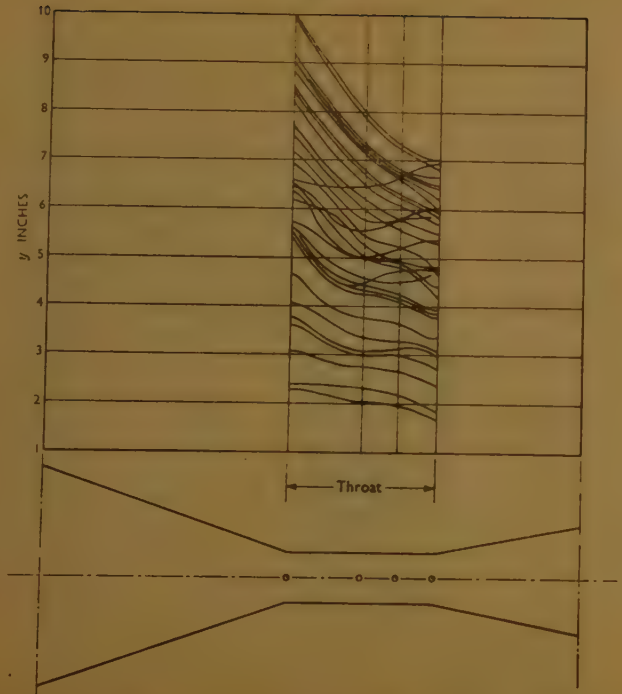
WATER PROFILES IN A RECTANGULAR THROAT, 4·53 INCHES BY 24 INCHES



Figs 11

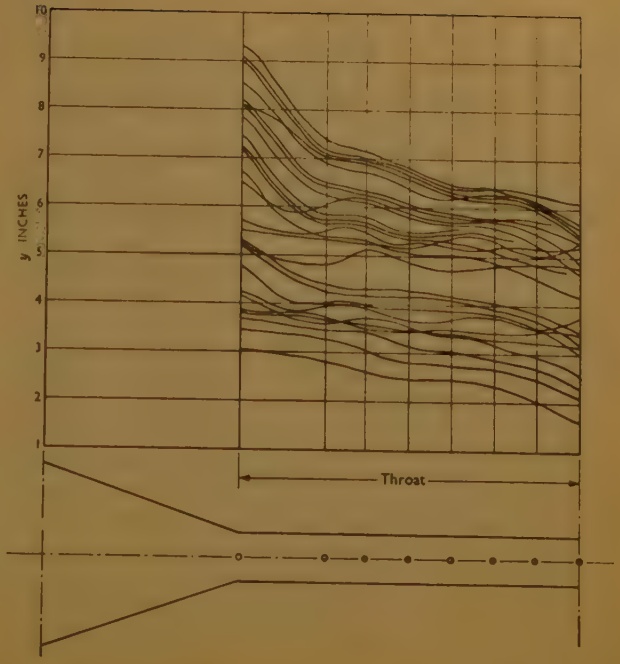
WATER PROFILES IN A RECTANGULAR THROAT, 4·53 INCHES BY 12 INCHES

Figs 13



WATER PROFILES IN A PARABOLIC THROAT ($f = 0{\cdot}15$ INCH ; $L = 12$ INCHES)

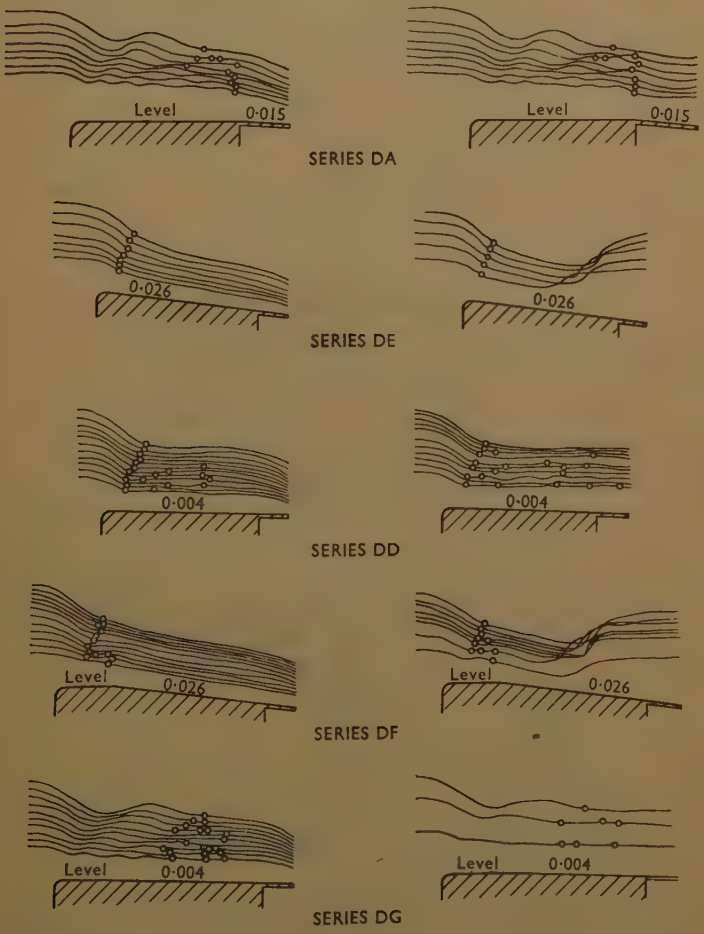
Figs 14



WATER PROFILES IN A PARABOLIC THROAT ($f = 0{\cdot}15$ INCH ; $L = 28$ INCHES)

chosen the position of the downstream gauge, or control section. Furthermore, in view of Woodburn's and Webb's experiments, why had the Author selected the particular upstream and downstream slopes of his triangular weir ? Was it certain that there would always be a low-pressure

Figs 15

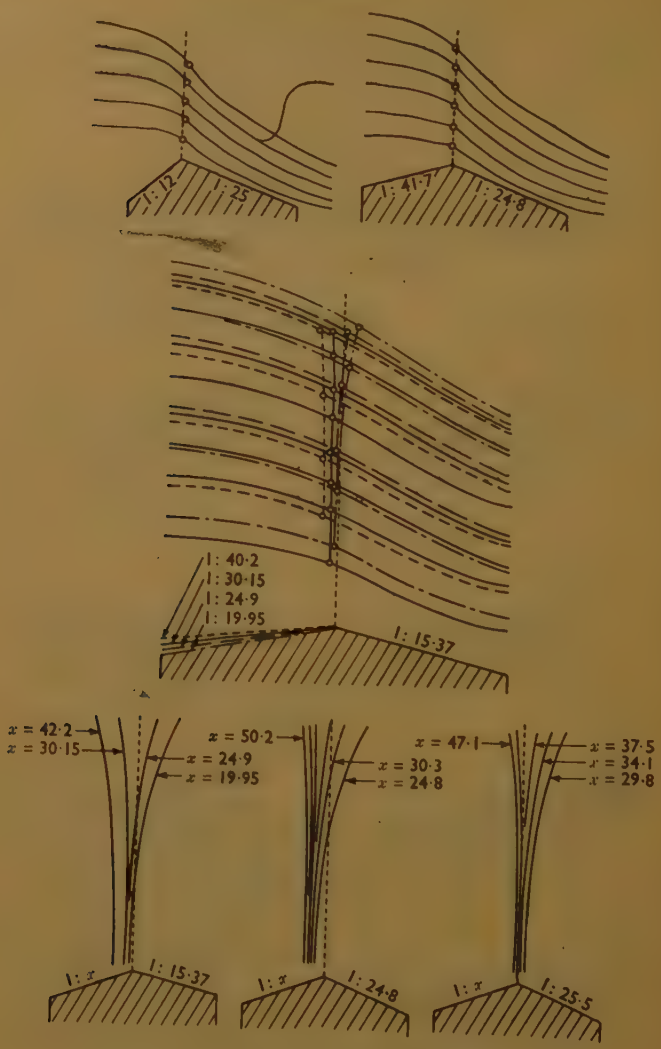


Woodburn's Experiments on Broad-Crested Weirs

pocket of eddying water with its highest point at the same position or control section, even at low rates of flow ?

In carrying out the experiments, four manometers having their zero at crest level had been used in recording the values of h, P, T, and H.

Figs 16



Critical Depth Loci (Webb's Experiments)

The technique employed by some investigators for determining the zero of the manometers had been to empty the channel until the water surface was apparently at crest level of the weir, ignoring surface tension of the water. Dr Kansoh hoped that the Author had taken that into consideration and had used some instrument similar to that used for determining the zero of a notch.

The Author had provided in each of two piers a single Pitot orifice which "should be at crest level." For what reason was a single orifice to be made at crest level in both piers? Was such an orifice going to give a representation of the average or mean value of H' under all conditions?

In Dr Kansoh's opinion, such an orifice should be placed opposite the locality of the average or mean velocity of the stream—for example, at a certain depth below the water surface down to the centre line of the channel; or alternatively, a series of orifices should be used.

Therefore, it was not to be expected that the corresponding values of T and H' in the Author's experiments would agree with one another. Excluding the value $H' = 2{\cdot}667$ of the sixth run in Table 1 (which was due to a printer's error), it was to be observed that the values of H' were greater than the corresponding values of T in the first four Tables (pp. 235 to 237), except in the third and fourth runs of Table 1, the runs Nos 25, 26, and 27 of Table 3, and the first run of Table 4, while the values of H' were less than the corresponding values of T in Table 5, with the exception of runs Nos 7 and 8, and in Table 6, except in the sixth run.

It was true that the difference between H' and T was small, but it was equally true that in not one of the 81 experiments did H' have the same value as T. It seemed that, after all, the proposed weir was but another of the double-gauge methods with most of the "complexities and liability to error."

The Author, in reply, observed that Messrs Lacey, Horner, and Tiffen had criticized his disregard of the results obtained with the two lower discharges of Tables 1 and 2. Comparing results obtained in the modular range with the proposed standard values of 3·580 for C_T and 3·456 for $C_{H'}$ Tables 1, 2, and 3 gave the results shown in Table 8.

TABLE 8

Table	Q	T: inches	C_T	Difference: per cent	H'	$C_{H'}$	Difference: per cent	No. of observation
1	0·577	2·644	3·347	−6·5	2·665	3·307	−4·3	(1)
2	0·793	3·238	3·433	−4·1	3·276	3·335	−3·5	(1)
3	1·172	4·077	3·551	−0·8	4·111	3·507	+1·4	(6)

The Author had assumed that practical engineers would seldom be interested in values of T or H' less than 4 inches, and had considered that the results obtained for the three higher discharges justified the simplification of adopting for general use the constant value for C_T proposed by him. In cases where discharges fell to give T a value less than 4 inches and where it was important that those low discharges should be accurately measured, the necessity for adopting smaller-than-standard values for C_T or C_H, was manifest.

Under modular conditions in the Author's tests, T had not exceeded a value of 0·4 foot, and a more important question arose as to what extent the Author's standard coefficients would hold good in cases where T might be as much as 10 feet, or even more. In that connexion the Author would invite attention to results obtained in 1899 at the Cornell University hydraulic laboratory on a very large model, the profile of which differed only very slightly from that used by the Author. The faces of the Cornell model (Series 9) had the same slopes as the Author's model, but the crest instead of being sharp had a relatively narrow flat 0·33 foot wide; the height F above bed was 4·94 feet and the highest value of H' was more than 5 feet. Values of the coefficient as recomputed by Robert E. Horton[7] were directly comparable with the Author's $C_{H'}$. Horton's values (after correcting an arithmetical error in No. 3) were as shown in Table 9, and indicated that the error arising from using the proposed standard values of C_T and $C_{H'}$ for large weirs would not be excessive.

TABLE 9

No.	H': feet	$C_{H'}$	Percentage in excess of 3·456
1	5·0992	3·575	+3·3
2	4·2116	3·525	+2·0
3	3·3747	3·465	+0·3
4	2·5573	3·482	+0·8
5	1·6950	3·557	+2·9
	Average	3·521	+1·9

In *Fig. 6* Mr Lacey had presented an interesting analysis of the nature of the standard curve of *Fig. 4*. For $P/T = 0{\cdot}90$, equations (10) and (11) gave C_T the same value. Compared with the tabular values of *Fig. 4*, salient discrepancies were given in Table 10.

In high floods with P/T approaching unity, the effective jet after crest-contraction would flow in a surface-stream without curvature. In those circumstances the velocity at the *vena contracta* would be $\sqrt{2g(T-P)}$ and equation (10), which gave the discharge intensity as:

$$q = C_T \,.\, T^{\frac{3}{2}} = 6{\cdot}85\,\sqrt{(T-P)}T = (0{\cdot}854T)\,\sqrt{2g(T-P)}$$

indicated a contraction of the jet to a depth of 85·4 per cent of T.

In reply to Mr Wileman, the Author had had no experience of British rivers, but on Punjab canals, where measuring weirs were usually attended and recorded daily, he had frequently used Pitot-piers without experiencing the troubles envisaged by Mr Wileman. On British rivers, the Author considered that it would be safe to use Pitot-piers at sites where debris could be cleared by a daily attendant. Pending experience so gained,

TABLE 10

P/T	C_T		Difference : per cent	Equation No.
	Tabular	Calculated		
0·26	3·580	3·571	−0·2	
0·37	3·458	3·430	−0·8	
0·47	3·300	3·285	−0·4	(11)
0·75	2·776	2·722	−1·9	
0·90	2·124	2·165	+1·9	
0·90	2·124	2·166	+2·0	
0·91	2·052	2·055	+0·1	(10)
0·92	1·960	1·937	−1·2	
0·97	1·156	1·186	+2·6	

general adoption of Pitot-piers appeared inadvisable. Regarding the piezometer holes, the Author considered that, while there would be little risk of them failing to operate by collecting leaves or submerged vegetation, gradual choking of the duct by small solid particles was inevitable, so that occasional flushing of the duct by pouring water into the float-chamber would be necessary. For this to be effective, provision would have to be made for a removable watertight plug of adequate diameter screwed into the metal of the duct and flush with its surface.

Whilst it was possible to devise a mechanism which, actuated by the two floats, would directly record the discharge, that would be costly and liable to unavoidable error owing to friction and backlash. For that reason the Author had contemplated the use of two separate instruments of the usual simpler type for recording P and T respectively, leaving it to the engineer to calculate the discharge by means of the Tables in *Fig. 4*.

As Mr Wileman had suggested, by raising the level of its crest a weir having the standard profile of the Author's model could be rendered modular up to any stage of discharge that the designer chose. A single recorder would then give the discharge over the modular range of operation, leaving the engineer to assess by current meter the discharge value of readings recorded in the non-modular range of operation. For accurate measurement of low discharges, Mr Wileman had also suggested a depressed weir of suitable width flanked by submersible side-walls. Those side-walls would have to be carried, with some rounding, into the concrete-covered side-slopes of the main channel; they would thus form the crests of the two side-weirs which might be designed to spill on to a concrete floor at channel-bed level.

In reply to Mr Horner, the Author had used the term " long-throated " to emphasize the need for a long prismatic throat required to obtain a high modular limit, and to include throats of any cross-section.

Provided the throat were long the second reading would, as Mr Horner

had stated, always be taken in the throat. If, however, there were no throat—for example, in the case of a sharp-edged weir—the only alternative would be to take the second reading in the tail-water, which the Author had suggested as being the usual practice.

Mr Horner's next point had already been dealt with.

In presenting *Fig. 3* and selecting Tables 4 and 5 for the purpose, the Author's object had been to ascertain the modular range of his model and the effect on the coefficient $C_{H'}$ of a raised bed. His intention had been, not to claim constancy of the modular coefficient for different discharges, but to verify its constancy—within 1 per cent up to 80 per cent submergence—as indicated by the last seven results of Table 4.

For very small flows, with T less than about 4 inches, the Author realized the necessity of adopting a modular coefficient smaller than the standard value.

He had recognized the need for occasional flushing to remove silt from the piezometer ducts. The same precaution applied to the Pitot-duct, and for that he suggested placing a removable flushing-plug in the nose of the pier a few inches above the Pitot-hole.

In compiling Table 7 Mr Cresty had credited standing-wave flumes with a modular limit of 94 per cent, and a Crump weir with only 70 per cent. The Author claimed that his *Fig. 3* fully justified raising the latter limit to 80 per cent—thereby reducing the minimum working-head from 27 to 18 inches. For a standing-wave flume he considered that a limit of 94 per cent would call for an extremely elaborate and costly design and could only be adopted with safety if the outfall channel were stable and its stage-discharge relationship known with precision. For a normally good design, a limit of 90 per cent would be a more reasonable figure, and even that figure would require slow divergence in bed and sides to ensure recovery of pressure-head. A modular limit of 90 per cent gave a working head of 12 inches for the 10-foot flume, and of 9½ inches for the 14-foot flume. The Author would never dream of putting a 33-foot weir into a 20-foot channel and would be quite content with the 16½-foot alternative of Table 7. At maximum discharge that would operate with a working-head considerably smaller than could be safely allowed for either of the two flumes and, being very much shorter in axial length, would certainly require much less concrete work.

Replying to Dr Kansoh the Author observed that single-gauging required the meter to be modular and that, in that case, suggestions (2) (widening the modular range) and (3) (reducing the limiting modular afflux) had the same meaning. The Author was of the opinion that the standing-wave flume or weir was a device upon which it would be difficult to improve in regard to features (1), (2), and (3). For that device to have a high modular range, of 90 per cent or more, the Author considered that a long throat was essential and that suggestion (4) (reduction of cost by shortening the throat) could only be attained at the price of a reduced

modular range. In other words: improvements (4) and (2)–(3) suggested by Dr Kansoh were mutually incompatible.

On p. 227 of the Paper, equation (1) expressed the fact that Q depended upon both pressure and curvature. For the hypothetical variation dQ of equation (2), if r was constant p was variable, and vice versa. The Author was at a loss to understand what Dr Kansoh meant by "some other mean effective radius."

The Author was also unable to grasp the significance of the expression for Q given by Dr Kansoh.

In *Figs 9* to *14* the position judged to be the control-section had not been indicated. In *Figs 15*, only the weir of Series DA had a long crest, and in that weir the sharp upstream corner would cause "separation," giving a coefficient less than the 3·09 of a gently curved approach. All the weirs of *Figs 16* were angular and would behave like the Author's model, where the true control section was not at the crest, but downstream of it, so that conditions of flow were complicated by curvature and contraction of the effective jet.

The Author had assumed that the pressure of the eddy, as measured by his P-gauge, would be much the same at different points in the eddy, and that assumption appeared to be confirmed by the fact that modular values of P/T were sensibly the same for different values of T. He had chosen the two slopes with the dual object of obtaining a low value of the modular ratio P/T and of attaining a moderately wide modular range.

Dr Kansoh could rest assured that due care was taken to ensure correct "zero readings" on the four manometers.

The Author had deemed it advisable to use a single Pitot orifice in a standard position that could be reproduced in prototypes. To cover low discharges the orifice had to be no higher than the crest. In that position the value of T recorded was admittedly less than the average kinetic head of all filaments and therefore less than H', except in the case of Table 5 where the raised bed had a larger effect on T than on H'.

References

1. J. G. Woodburn, "Tests on Broad-Crested Weirs." Trans. Amer. Soc. Civ. Engrs, vol. 96 (1932), p. 387.
2. See ref. 1. Appendix by A. R. Webb, "Supplemental Tests—Weirs with aprons inclined up stream and down stream," (p. 408).
3. See ref. 1. *Discussion* by B. A. Bakhmeteff, p. 423.
4. H. K. Palmer, "Adaptation of Venturi Flumes to Flow Measurements in Conduits." Trans. Amer. Soc. Civ. Engrs, vol. 101 (1936), p. 1195. *Discussion* by Hunter Rouse, p. 1218.
5. R. L. Parshall, "The Improved Venturi Flume." Trans. Amer. Soc. Civ. Engrs, vol. 89 (1926), p. 841. *Discussion* by J. Hinds, p. 859.
6. See ref. 5. *Discussion* by J. C. Stevens, p. 872.
7. "Weir Experiments, Coefficients and Formulas." Paper No. 200, Dept of the Interior, U.S. Geological Survey. Washington Govt Printing Office, 1907.

CORRESPONDENCE

On Papers published in Proceedings Part I, May 1952

Paper No. 5844

"A 75-inch-diameter Water Main in Tunnel: a New Method of Tunnelling in London Clay" †

by

Peter Adamson Scott, B.Sc., M.I.C.E.

Correspondence

Mr E. W. Cuthbert referred to the Author's statement on p. 314 that consideration had been given to the application of a lining by mechanical means to the steel main after laying. Such a lining had been applied to 90 feet of ¼-inch-thick steel main of 77½ inches internal diameter shortly after the works described in the Paper had been completed. Mortar had been employed consisting of equal parts of normal Portland cement and

Fig. 14

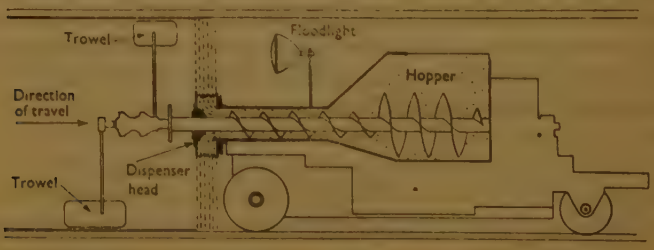


DIAGRAM OF "CENTRILINE" MACHINE FOR LINING PIPES FROM 36 INCHES TO 48 INCHES DIAMETER

(This Figure is based on a diagram appearing in "Reconditioning of Trunk Mains in the City of Montreal," by Frank Y. Dorrance.)

dry sand passing No. 16 sieve, mixed with just sufficient water to give the correct consistency for it to adhere to the pipes.

The lining machine, shown diagrammatically in *Fig. 14*, had consisted

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 302 (May 1952).

of an electrically propelled truck which ran on three rubber-tired wheels in the invert of the unlined pipe. A hopper on the truck had held about $3\frac{1}{2}$ cubic feet (4 minutes' supply) of mortar. A slotted dispenser head at axis level of the pipe and rotating at 1,500 revolutions per minute had been fed from the hopper by a worm screw, and the mortar had been sprayed by centrifugal force evenly around the circumference of the pipe. The thickness of the coating had been controlled by adjusting the speed of the truck through the pipe. It had been found that a $\frac{1}{2}$-inch-thick lining could be placed at the rate of 1 foot run of pipe per minute. At the rear of the dispenser head spring-loaded steel trowels on the ends of slowly rotating arms had rendered the surface of the lining smooth. An operator riding on the machine had supervised the filling of the hopper and steered the leading wheel to overcome any tendency of the machine to ride up the wall of the pipe.

Mortar had been mixed at the ground surface in a special mixer having a fixed drum and paddles rotating on a horizontal shaft. After mixing, the mortar had been lowered by crane in a special skip to the foot of the shaft, where it had been tipped into a hopper arranged to discharge into small rubber-tired "buggies." The "buggies" had been propelled by hand to and from the machine. Thirteen men had been required for the whole operation.

The lining had been cured in a humid atmosphere by allowing water to stand in the invert of the pipe. Inspection of the surface of the lining had revealed a few irregularities, consisting mainly of fine ripple marks and score marks—apparently caused by vibration and irregularities of the trowels. There had been some uneven places apparently caused by the mortar locally being a little too stiff. In no place had the lining been found to be loose, unsound, or less than $\frac{7}{16}$ inch thick. At junctions between new and old lining the joints had had to be finished by hand.

The machine had been operated by the Centriline Corporation of the United States, John Mowlem & Co. Ltd being their agents in Great Britain for the work described.

Mr A. C. Buck observed that he was particularly interested in the Author's account of the tunnel, quite apart from the most ingenious and successful form of pre-cast concrete outer lining described, because of his association a few years ago with a scheme similar in some respects to the present one. During the construction of a tunnel of 19 feet internal diameter, forming part of a hydro-electric scheme at Lake Tekapo, New Zealand, it had been considered necessary to render absolutely watertight the last 600 feet of its length. The tunnel had been driven by means of two shields in glacial moraine, and was lined with pre-cast concrete blocks in much the same way as had been employed in a recent London tube extension.

The most positive means of ensuring watertightness was to use a steel lining, but tonnage had to be kept to the minimum. It had been decided

to use $\frac{3}{16}$-inch plate for the inner 300 feet and $\frac{1}{4}$ inch for the remainder. Because of the large diameter some doubts had been felt as to the practicability of such thin plate owing to excessive flexibility, but doubling the thickness would not have helped much, and in any case it had been considered that the answer to the problem lay in the method of support given to the lining during handling. A design for a stiffening ring providing continuous support around the circumference had been included in the tender as a proposal, and had been adopted by the contractor. The ring had been composed quite simply of three light wooden trusses connected at their ends by expanding-screw joints.

The contractor had tackled his problems in a workmanlike spirit and had showed that, sensibly handled, the thin plate presented no special difficulties and in fact had some advantages. A contributing factor to that result had been the use of the trusses which had behaved very well. All joints in the plate had been butt-welded. A full description of that work as seen from the contractor's viewpoint had been published.[1]

The space between the steel lining and precast blocks had been filled with pumped concrete and it had been found that flats sometimes formed in the $\frac{3}{16}$-inch plate at pumping points. It had been impossible to push those out as they formed and it had not been considered worth while to cut them out since, owing to their shape, no measurable increase in resistance to the flow of water would be offered nor were they, in fact, very obvious to the eye. With the $\frac{1}{4}$-inch plate that trouble had disappeared.

There was one aspect of concrete-backed steel linings which might be important. It appeared to be common for the bond to fail between steel and concrete in localized patches. Those patches may be detected by lightly tapping with a hammer, the difference in sound being quite marked. Any gap thus formed was certainly extremely small, since an attempt to pump grout into a "drummy" area had failed. But there did seem to be the possibility that water would penetrate to those areas and set up corrosion. Thickening the plate did not appear to reduce the incidence of that failing.

It would be interesting to know why an internal lining of concrete had been preferred to bitumen, which was now widely used in steel watermains. A $\frac{1}{4}$-inch thickness of bitumen would offer a saving of $1\frac{1}{2}$ inch on the diameter of the main.

It seemed likely that the 4-inch average clearance at the spigot-and-socket joints could, with experience, be reduced. The contractor at Tekapo had had to make do with less at certain points, but the decision on the clearance to be used ultimately had to be a compromise between ease in installation and economy of material.

Had any attempt been made to estimate the clay pressures likely to

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[1] J. Forrester, "Steel Tunnel Lining and Penstock, Lake Tekapo." *New Zealand Engng*, vol. 6, No. 1, p. 12 (Jan. 1951).

be exerted upon the concrete lining and, if so, had the thickness of the lining been calculated ? The method used at Tekapo had been the one used to estimate the thickness of linings generally but as a check upon its stability a number of pressure gauges had been built in as the work progressed, with the purpose of determining the distribution of pressure around the circumference, and the variation of pressure with time. The tunnel had about 250 feet of cover above the gauges, whilst maximum pressure recorded had been equivalent to about 20 feet head of soil. After a few weeks, pressures had appeared to become stabilized. That was in a uniform soil of considerable depth composed of all fractions from clay to coarse gravel and boulders. On the other hand, measurement of strains in a London tube tunnel, lined with cast-iron segments, had suggested that within a week or two of installation the pressures acting had come from the full cover of clay.

The steel lining had no doubt been adopted only after consideration of other forms of lining. Would the Author be good enough to indicate his objections to the use of reinforced or pre-stressed spun-concrete pipes ?

The Author, in reply, observed that it had perhaps not been made clear that the experiment in concrete lining in situ, described in Mr Cuthbert's contribution, had in fact been carried out on a section of the main described in the original Paper. The success of the experiment opened the way to a great reduction in the weight of pipe to be handled as well as a reduction in the amount of steel required, and the method was likely to be given further consideration.

Mr Buck's description of the successful lining with $\frac{1}{4}$-inch plate of a 19-foot tunnel in New Zealand was extremely interesting and proved that the fears entertained with regard to lining of a similar thickness for the 75-inch tunnel were unfounded, although the problem of providing adequate stiffeners during erection in such a small tunnel was complicated by the lack of working space. It was assumed that in the case of the 19-foot tunnel, the pre-cast concrete lining had been designed to withstand any outside pressure.

The concrete lining to the 75-inch-diameter water main had been adopted to provide added stiffening for the prevention of buckling as well as for durability. Although a spun hydrocarbon or bitumastic lining was commonly used in such mains, the concrete-lined pipe had been found to have a very long life, a point which in that case was a matter of considerable importance.

In any future extension, it was probable that the clearance at the joints between pipe and Donseg lining would be reduced, which would result in a saving in the amount of pumped concrete required. The steel pipes had to be designed on the basis of their having to be drawn through as much as 1 mile of tunnel, and the amount of clearance required had to be found by experiment.

In designing the 75-inch main it had been assumed that, as Mr Buck

had said, the external pressure on the tunnel would arise from the full cover of clay.

Much consideration had been given to both reinforced and pre-stressed concrete pipes, both pre-cast and cast in situ, but handling problems of pre-cast pipes and, in particular, a satisfactory solution to the problem of effecting a reliable joint between sections had led to the rejection of those types; in the case of pipes or reinforced lining formed in situ, the problems arising from the restricted working space and the probable rate of progress had been found to be insoluble.

Paper No. 5847

"The Use of Compression Waves in Deep-Well and Borehole Pumping" †

by

Eric Crewdson, M.C., B.Sc., A.M.I.C.E.

Correspondence

Mr W. P. Steele observed that the Author had revealed an interesting development in the technique of pumping from deep boreholes, which should simplify to no small extent mechanical problems at present attendant upon the use of the ordinary cylinder with the plunger operated by long wooden rods. Trouble sometimes arose with that pattern in countries where earth-tremors were encountered, the rising main and rods getting out of line and jamming. Since there were no moving parts in the rising main with the type of pump described by the Author, accidents of that kind were not likely to occur.

By all accounts, however, there was a certain limitation to the depth of pumping, using standard-size steel piping for the rising main, since when using a galvanized wrought-iron rising main to B.S.S. 788 it appeared that the maximum depth of pumping permissible would be approximately 690 feet, corresponding to half the test pressure of 600 lb. per square inch. To allow for peak pressures developed during surging, however, the actual depth would appear to be rather less than that. Would the Author confirm that point? It was admitted that pumping from depths greater than about 500 feet, even with the use of rod-actuated cylinders, became expensive.

It was perhaps unfortunate that the Author gave so little detail of the mechanically-operated delivery valve (valve A in *Fig. 1*), since that obviously had a critical bearing on the surge pressures developed. The

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 318 (May 1952).

Author had remarked that the use of a poppet valve for that purpose had proved ineffective, although the reason why that should be so was not made entirely clear. The sudden closing of a poppet-type valve by means of a steep-faced cam would seem to be an effective means of developing the required rapid rise in pressure for a steep-fronted wave of pressure. That delivery valve would act in the same manner as the " flash-valve " of a hydraulic ram, and it was made quite clear in a recent paper[1] by Krol that the drag coefficient of, and the loss of head by turbulence in, the flash-valve were two of the three critical factors governing the theory of operation of hydraulic rams. The third factor, the loss of head during retardation of the water-column in the drive pipe would have a much smaller value in the Author's type of pump than in the hydraulic ram, in which a moving column of water had to be brought finally to rest to develop the required pressure-rise.

The theory of operation of the hydraulic ram was given in a very satisfactory manner in Dr Krol's paper, but there remained the empirical determination of the three critical factors quoted above for any given ram installation before the installation could be designed adequately. A workable theory of operation of the hydraulic ram had been developed earlier for a particular design of ram by M. P. O'Brien and J. E. Gosline.[2]

Would the Author give a clearer description of the actuation of the finally adopted design of delivery valve in his pump ?

There was clearly room for much study in developing the analogy between the operation of the compression-wave pump and the hydraulic ram along the lines laid down in Dr Krol's experiments, and it was to be hoped that a more concise theory of the operation of the compression-wave pump would be developed from that.

Dr M. M. Kansoh, of Cairo, observed that the Author had claimed that there was " no wholly satisfactory theory of the working of the compression-wave pump." The following, Dr Kansoh believed, was a satisfactory explanation of the working of the pump.

Each complete cycle of operation of the pump comprised three phases. At the beginning, the piston would be in its highest position and the pressures in the system would be as shown in *Fig. 6* (*a*). The first phase began when the piston started to move downwards and continued, as the Author had stated, until the water in the cylinder and in the drive pipe down to the foot valve was fully compressed, a pressure p being superposed on the initial pressures (*Fig. 6* (*b*)).

It was obvious that the water would be subjected to a gradually increasing compression so long as the piston was moving downwards. That meant that that phase occupied the time necessary for the crank to

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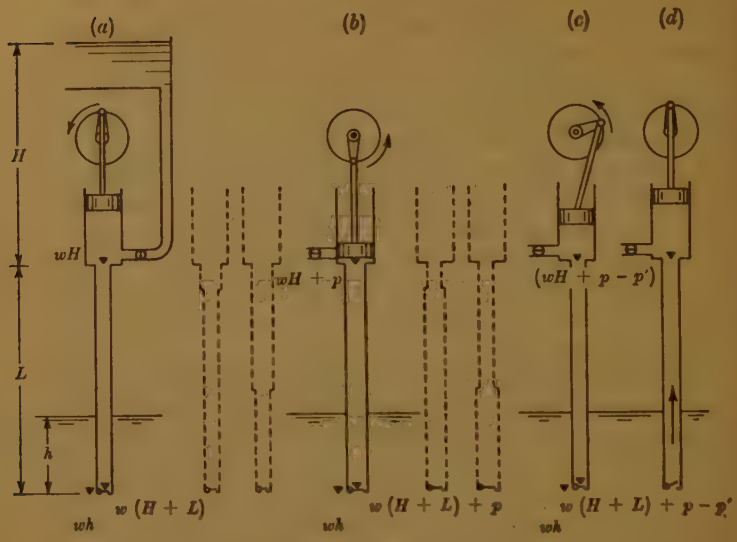
[1] J. Krol, " The Automatic Hydraulic Ram." Proc. Instn Mech. Engrs, vol. 165 (1951). (W.E.P. No. 64, p. 53.)

[2] M. P. O'Brien and J. E. Gosline, " The Hydraulic Ram." Univ. of Calif. Engng Pubn, 1933.

Figs 6



describe one-half of a revolution (not one-quarter of a revolution as stated on p. 320), irrespective of the length of the drive pipe or the velocity a of propagation of sound or a pressure wave in water.

Therefore, time taken for the first phase $= \frac{1}{2} \times \frac{60}{N}$ seconds and, in the solved example, the first phase occupied $\frac{1}{2} \times \frac{60}{352}$, or about 0·0857 seconds, and not 0·0343 seconds.

The second phase started when the delivery valve was opened (which was supposed to happen suddenly). A wave of low pressure, p, travelled downwards at a velocity a and reached the foot valve after $\frac{L}{a}$ seconds (*Fig. 6 (c)*).

At the end of the second phase, and the commencement of the third, the foot valve was opened automatically, the water rushed in from outside, and the column of water moved bodily upwards, impelled by the difference between the pressure (wh) outside the foot valve and the new pressure ($w\,(H + L) + p - p'$) just inside it (*Fig. 6 (d)*). That difference was :

$$wh - [w(H + L) + p - p'] = w(H + L) + \text{head loss.}$$

Therefore :

$$h = 2H + 2L + \frac{p}{w} - \frac{p'}{w} + \text{head loss.}$$

The moving column of water had a weight (not a mass, as stated by the Author) of W lb., where :

$W = w \times l \times$ cross-sectional area of the pipe.

The force F in lb., opposing the upward movement of the column was equal to W (in lb.) + [(delivery head—depth of water outside the drive pipe (in feet)) × 0·434] × cross-sectional area of pipe (in square inches).

$$F = \text{mass} \times \text{acceleration} = \frac{W}{g} \times f$$

Therefore :

$$f = \frac{g}{W} \times F.$$

The distance S, in feet, by which the water column rose at each pumping stroke or, more precisely, at each revolution, would be :

$$\left(\frac{\text{gallons per minute}}{\text{revolutions per minute}} \times 0{\cdot}1605 \right) \div \text{pipe area (in square feet)}$$

$$= \tfrac{1}{2} f t^2 = \tfrac{1}{2} \left(\frac{g}{W} \times F \right) t^2$$

Therefore the time t taken for the third phase was $\sqrt{2 \times \frac{S}{f}}$ seconds, and the time taken for all three phases, or for one revolution of the crank would be :

$$\frac{60}{N} = \tfrac{1}{2} \times \frac{60}{N} + \frac{L}{a} + \sqrt{2 \times \frac{S}{f}}$$

The velocity a, taken in the example given as 4,080 feet per second, might appear to be low in comparison with the velocity of sound in water $\sqrt{\frac{K}{\rho}}$, which was known to be 4,710 feet per second ; but taking into consideration the elasticity of the pipe of 4 inches diameter with an assumed steel wall thickness of $\frac{1}{8}$ inch, the velocity a would be :

$$\sqrt{\frac{K}{\rho} \div 1 + \left(\frac{K}{E} \times \frac{4}{\frac{1}{8}} \right)} = 4{,}100 \text{ feet per second,}$$

or, more accurately :

$$a = \sqrt{\frac{K}{\rho} \div 1 + \frac{5}{4} \left(K \times \frac{4}{\frac{1}{8}} \right)} = 3{,}980 \text{ feet per second.}$$

The latter formula took into account both the longitudinal and circumferential elasticity of the pipe. Therefore :

$$\mathrm{N} = \tfrac{1}{2} \times \left[60 \div \left(\frac{L}{a} + t \right) \right]$$

The values of the speed N, calculated on the basis of the above theory,

were not in exact agreement with the measured ones, probably owing to the not completely instantaneous opening of the delivery valve or to inaccurate readings, or both, but they were definitely more accurate than those calculated according to the Author's theory or from the formula :

$$N = \frac{3{,}900}{\sqrt{L}}$$

which was meaningless since it gave one value of N for each length L of the drive pipe, whereas in fact, for the same length of pipe, the speed might vary widely, as was indicated in Table 3.

TABLE 3 *

	Gallons per minute	S	t	0·0343 + t	N (calculated)	3 × 0·0343 + t	N (Crewdson)	N (measured)	η
1	5·4	0·0310	0·0433	0·0776	386	0·1462	410	320	35·9
2	7·2	0·0399	0·0489	0·0832	360	0·1518	396	332	44·4
3	8·4	0·0450	0·0521	0·0864	348	0·1550	387	344	49·8
4	8·4	0·0439	0·0515	0·0858	350	0·1544	388	352	50·3
5	4·4	0·0225	0·0369	0·0712	421	0·1398	429	360	42·5
6	10·0	0·0604	0·0659	0·1002	300	0·1688	356	305	36·1
7	11·4	0·0652	0·0685	0·1028	296	0·1714	350	320	41·4
8	12·0	0·0650	0·0684	0·1027	296	0·1713	350	340	44·9
9	12·6	0·0665	0·0690	0·1033	295	0·1719	349	349	45·1

* In these calculations, a = 4,080 feet per second.

The pump would function at its best if it could pump water against a given head H, with a suitably chosen length L for the drive pipe, from a depth $L - h$ as great as possible. The value of h or of $2H + 2L + \frac{p}{w} - \frac{p'}{w}$ + head loss should preferably be as small as possible. H and L were supposed to be already fixed. The losses could be decreased by taking

Fig. 7

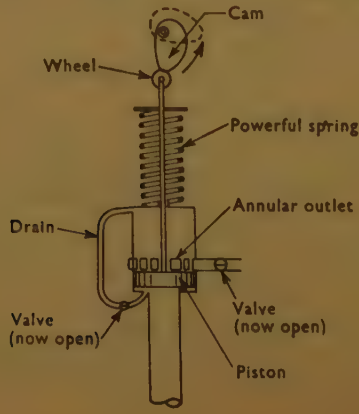


TABLE 4 *

	Length L	Diameter: inches	H	h	W	F	f	Gallons per minute	S	t	$\frac{L}{a}+t$	N (Calculated)	N (Crewdson)	$N=\frac{3,900}{\sqrt{L}}$	N (Measured)
1	55·1	2	26·9	54·6	75	111·0	47·6	3·16	0·051	0·0463	0·0598	510	699	526	455
2	70·0	3	60·0	40·0	215	306·9	46·0	5·50	0·039	0·0413	0·0584	515	647	466	460
3	98·5	3	16·0	74·5	302	308·1	32·8	8·48	0·069	0·0650	0·0891	337	433	393	400
4	140·0	4	21·0	123·0	764	785·8	33·2	8·40	0·0439	0·0514	0·0857	350	388	330	352
5	153·5	3	60·0	115·5	470	537·5	36·8	7·10	0·072	0·0626	0·1002	299	346	315	322
6	253·5	3	90·0	150·5	780	740·2	30·6	3·93	0·054	0·0595	0·1115	269	246	245	238

* In these calculations, $a = 4,080$ feet per second.

suitable precautions; p need not be excessive and p' should be made maximum.

The magnitude of p' depended on the speed and amount of opening of the delivery valve.[1]

It was essential for the pressure to be released suddenly, and for that purpose Dr Kansoh proposed the modification shown in *Fig. 7*.

Mr P. Linton observed that a comprehensive theory of the compression-wave pump, taking into account the various losses, was likely to be very complicated. A treatment of a special case, using the graphical method of surge calculation, might perhaps suggest a way of attacking the problem. The same method had been used by Bergeron[2] when dealing with a reciprocating pump without air vessel.

The diagram in *Figs 8* related to a drive pipe of length L such that the time taken by a surge wave to travel from the pump to the foot valve and back ($2L/a$) corresponded to a crank travel of 90 degrees. The main part of the Figure showed a distance/time diagram of such a surge wave and the conditions at the pump and at the foot valve when the surge arrived at either end and was reflected there. The right-hand diagram was the conventional surge graph for that particular surge wave. If the pump was to work at all, it seemed essential that the surge diagram should form a closed polygon covering either one or several revolutions of the pump; successive cycles would then be identical and the pump would operate smoothly. That condition led to certain limitations of that type of pump which could usefully be explored further.

Figs 9 showed the method applied to a practical case. The pump, of the same dimensions as the one described by the Author, ran at 240 revolutions per minute, and the 3-inch drive pipe was 136·5 feet long, so that $2L/a$ corresponded to a 90-degree crank angle. The total lift of the pump was 250 feet. The conventional surge diagram could be analysed to give the graphs of piston pressure and flow through the foot valve in relation to crank angle, as shown in *Figs 9*. When the delivery valve was open, the pressure at the pump would of course equal the delivery head of 150 feet (all losses being neglected). When the delivery valve was shut, the piston pressure was related to the piston velocity, which was shown in the surge diagram by the short vertical broken lines and expressed in gallons per minute. The pressure at the foot valve was −100 feet (referred to the pump) when it was open, and might reach very high values when it was shut. The inflow into the drive pipe took place during a crank travel of only 90 degrees in that particular case.

An obvious difficulty in the application of the theory arose when the surge travel time $2L/a$ was no longer a simple fraction of the time taken for

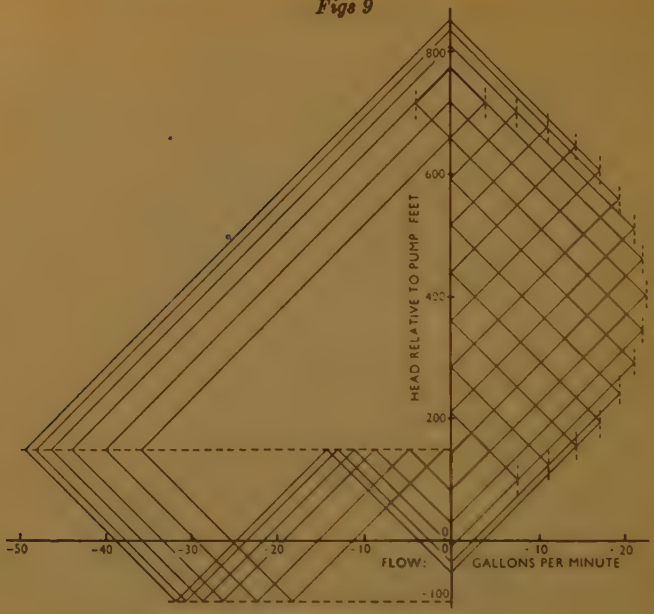
[1] A. H. Gibson, "The Mechanical Properties of Fluids." Blackie, London, 1946. See p. 228.

[2] L. Bergeron, "*Du coup de bélier en hydraulique au coup de foudre en électricité*" ("From water-hammer in hydraulics to the lightning stroke in electricity"). Dunod, Paris, 1950.

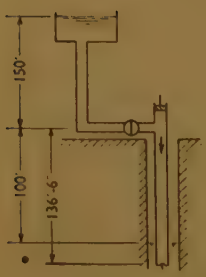
HEAD RELATIVE TO PUMP: FEET
800
600
400
200
FLOW: GALLONS PER MINUTE
+20
+10
-10
-20
-30
-40
-50
-100
SURGE DIAGRAM
300°
210°
120°
30°
Pump
Foot valve
TIME SECONDS
DISTANCE/TIME DIAGRAM

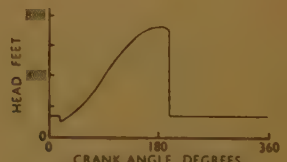
Figs 8

Figs 9

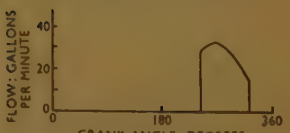


SURGE DIAGRAM





PRESSURE IN CYLINDER



FLOW THROUGH FOOT VALVE

one complete revolution of the crank. Although it seemed that the pump could only work when the speed and drive length were accurately matched, leakage and other factors might modify that rule in practice. It would be of considerable interest to obtain instantaneous pressure records on that pump, particularly near the piston, as well as a measure of the flow variation, and to compare them with the theoretical treatment suggested above.

Mr H. Kitson Chambers, of Perth, Western Australia, asked what provision was made for maintaining tightness of the joints. The effect of

impact vibrations, oscillations, and reactions of varying magnitude would have to be guarded against. How was that done ?

The Author, in reply, agreed that the maximum permissible depth of pumping would, as Mr Steele suggested, be limited by the strength of the drive pipe between the pump and the foot valve. The maximum depth adopted by the Author up to the present had been 220 feet from the water surface in the well to ground level. It was probable that for depths greater than 300 feet special quality piping would be required as well as a substantially strengthened construction of the pump itself.

The cam-shaped delivery valve was shown in two views in *Figs 10*, which the Author hoped would make its design clearer. The poppet valve used at one time by the Author had been quite as efficient as the cam-shaped valve. The drawback to its use had been that the cam operating it wore at a fairly rapid rate and trouble had been experienced with spring breakages. It also seemed to cause rather more noisy operation.

The Author found the theory of the hydraulic ram set out in Dr Krol's paper not wholly satisfactory. The quantity of waste water from the ram had been found to exceed that calculated by as much as 10 per cent in certain cases, and the discrepancy would have been noticeably increased had Dr Krol allowed in his theory for the reverse flow of water through the impulse valve which was bound to have taken place in his periods t_6 and t_7 and might also have taken place in period t_1, (see *Fig. 24* of Dr Krol's paper[1]). Moreover, it appeared that the theory was tested on only one length of drive pipe. It would have added greatly to one's confidence in the theory had Dr Krol tested it for the effect of varying the drive-pipe length.

Dr Kansoh's theory proposed for the pump was very interesting and it might well be that it would be found to give more satisfactory results than anything hitherto suggested. That was not, however, borne out by a study of Table 3. If the Author's admittedly illogical formula

$$N = \frac{3{,}900}{\sqrt{L}}$$

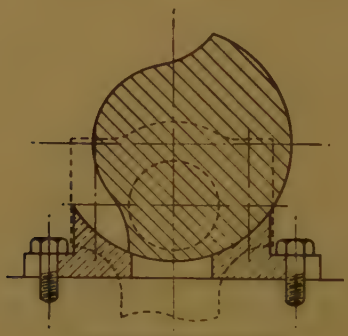
was applied, it gave for all the results in Table 3 a speed of 330 revolutions per minute, which was at most 7 per cent wrong compared with any of the measured speeds. Dr Kansoh's formula in the case of Test No. 5 gave a 17-per-cent error, and in Test No. 9 a 15-per-cent error.

Moreover, in the case of Test No. 5, even at 360 revolutions per minute the pump was clearly running too fast. The quantity pumped had dropped by 43 per cent and the efficiency by 15 per cent. Another test had been run at 369 revolutions per minute in which the quantity had dropped to 1·8 gallon per minute and the efficiency to 25·5 per cent. It would have been useless to raise the speed to 429 revolutions per minute as Dr Kansoh's theory suggested.

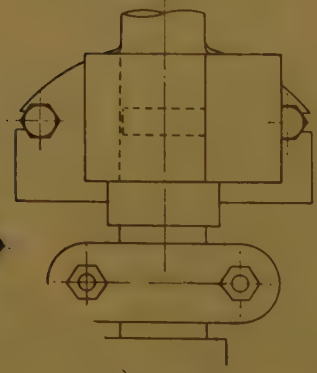
[1] See ref. 1, p. 773.

The Author had not seen the paper by Bergeron, referred to by Mr Linton, but was greatly interested in the possibilities which an attack on the problem on the lines of Mr Linton's contribution suggested. The Research Engineer of the Author's firm had applied Mr Linton's method to a number of actual tests of the pump's performance, but had found it to be in many cases an impossibility to get the diagrams to close. He was, however, carrying out further studies on those lines.

Figs 10



SECTION THROUGH VALVE



SCREEN VIEW
full-size

Owing to the inevitable leakages past the piston, and in the delivery valve and the air injector, it seemed likely to the Author that no wholly satisfactory theory of the pump's working could ever be realized.

In answer to Mr Kitson Chamber's question, the drive-pipe joints, which had to be absolutely airtight, were made with screwed couplings. The pipe lengths were long-screwed and faced at each end and arranged so that the ends butted in the centre of each coupling. Back nuts were provided at each end of each coupling. A proprietary jointing compound was used to ensure tightness of the threads.

CORRESPONDENCE

on Papers published in Proceedings Part I, September 1952

Paper No. 5771

"The Most Recent Dams by the 'Società Adriatica di Elettricità (S.A.D.E.)' in the Eastern Alps" †

by

Dott. Ing. Carlo Semenza

Correspondence

Mr A. C. Buck observed that it was a pleasure to see the imagination of the engineer achieve such graceful and effective dams. Evidently a great deal of thought had been given to them structurally; but aesthetically they had not been at all neglected, and very pleasing results had been obtained by the simplest means. The Author was to be congratulated upon the installation of a variety of instruments for investigating the subsequent behaviour of the dams. It had been said that all civil engineering works were examples of research work on a large scale and therefore it seemed surprising that only in recent times had serious attempts been made to check in detail the validity of a design. The arrangements described in the Paper were exceptional, judged by everyday practice. It was still customary for structures of importance to be built with nothing but a very superficial check upon subsequent behaviour. The installation of measuring instruments and arrangements for periodic inspection had yet to become established practice but the tide seemed to be turning that way. The Author had an initial advantage in that he belonged to an organization which both designed and operated the power stations.

There were several points of peculiar interest common to all or most of the dams. First, the trouble taken to achieve symmetry, a device leading to slenderer sections. Another point was that in no case had the dams been pierced by transverse holes; that might be entirely a matter of topography or it might be that the high stress concentration which would be created by their existence had tipped the balance in favour of other arrangements. The spillway over the crest and a free fall for the discharge was a rather daring arrangement with the merit of simplicity; presumably

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 508 (Sept. 1952).

no protection had been added to the bottom and sides of the gorges. Finally, steel reinforcement had been used. Many engineers engaged on the design of dams worked to the rule that only compressive stresses should exist in their dams under all conditions of loading. The Author, however, accepted the existence of tensile stresses—apparently as a matter of course—and in accordance with the usual assumptions steel reinforcement became necessary, leading to slenderer sections. What was the permitted working stress ?

Regarding the Pieve di Cadore dam there was the possibility that a solid or buttress-type gravity dam, anchored by pre-stressed steel cables to the rock, would show considerable economies over the form adopted. That was a construction which seemed to be exploited entirely by French engineers and, rather strangely, neglected elsewhere. Details of the early application of the method has been published.[1]

Blocks had been used to face the Valle di Cadore dam entirely for frost protection and were not intended to carry any of the load. Could not in-situ concrete have been made which was adequate for both purposes ?

Referring to the calculation methods employed, had the Author considered the use of the relaxation methods developed by Sir Richard Southwell ? On p. 365 of Sir Richard's James Forrest Lecture,[2] Slide 12 showed a grid framework for which the method of solution was described on p. 61 of his book.[3] It was possible to determine the moments in flat slabs by substituting a grid framework and it seemed likely that a solution could be found for a thin shell in the same way, except that the unit problem would have another expression introduced owing to curvature in one or two directions. With a symmetrical shell the solution would be greatly facilitated.

The Author, in reply, stated that the tensile working stress permitted by Italian regulations of 1931 for an arch dam were :—

For concrete rings : not more than 5 kilograms per square centimetre.
For reinforced concrete rings : not more than 8 kilograms per square centimetre (analysing the rings as reinforced sections).

The maximum permissible working stress in compression was one-fifth of the ultimate stress after 28 days and one-seventh after 90 days.

The Author was acquainted with Mr Coyne's method of anchoring a masonry dam to the rock, but he thought that such methods should be used only in the complete absence of any other method which was

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[1] A. Coyne, "The Construction of Large Modern Water Dams." *Structural Engineer*, vol. 15, p. 70 (Feb. 1937).

[2] James Forrest Lecture, 1948, "Relaxation Methods : An Engineering Approach to Computation." J. Instn Civ. Engrs, vol. 30, p. 351 (Oct. 1948).

[3] "Relaxation Methods in Engineering Science : A Treatise on Approximate Computation." Oxf. U.P., 1940.

satisfactory from the constructional point of view. That was not the situation at Pieve di Cadore.

The blocks used for facing the Valle di Cadore dam were not intended to carry any load, but only to protect the surface from deterioration—especially that which might result from frost. The calculations for the arch had therefore allowed only for the thickness of the interior reinforced arch.

The Author thanked Mr Buck for his reference to the "relaxation method." It had not been judged advisable to use it for the Italian dams owing to lack of experience of its particular application to dam design. It had been deemed wiser to employ experts who were proved specialists in the particular analytical problems involved.

Paper No. 5857

"Sewerage and Sewage Disposal in Sub-tropical Countries, with Special Reference to South Africa and Mauritius" †

by

Ernest John Hamlin, D.Sc., M.I.C.E.

Correspondence

Mr F. E. Bruce observed that the Author had rightly placed emphasis upon the close relationship between public health and the necessary engineering services such as sewerage and sewage disposal. It was an aspect which had too often been neglected in the past. Engineers had been so absorbed in the mechanics of their work that they had not always appreciated its principal object—the preservation of good health. The Author had done a valuable service by giving practical examples of how the engineer, the doctor, and other specialists could collaborate both in investigating the route of transmission of disease and in devising methods of cutting that route.

Logically, the engineer's work followed the investigations of the doctors and biologists. In practice, however, knowledge was acquired in a piecemeal fashion, and the demand for protection from disease could not wait until medical and epidemiological knowledge was complete. Hence there was a need for research of the kind described by Dr Hamlin in connexion with poliomyelitis, in which engineering control was an essential part of the biological investigation.

The engineer who participated in work of that kind, or was engaged

† Proc. Instn Civ. Engrs, Part I, vol. 1, p. 561 (Sept. 1952).

in any form of public health engineering, needed to be qualified to meet the doctor and the biologist on common ground. He should have a broad understanding of public health principles and practice and a sound knowledge of the biological processes on which his work was based. Without that knowledge, his efforts were liable to be mis-directed and wasteful of the limited resources available.

The need for well qualified public health engineers was particularly apparent in the warmer climates with which the Paper dealt, because of the much greater number of health risks which existed there, and because of the generally low state of development. The British Colonies offered much scope for the enlightened use of public health engineers.

The Paper had touched upon some aspects of the effects of temperature on the sewage disposal problem (p. 581), but some information on its bearing on the design and operation of treatment works in warm climates would be of interest. Most of the processes relied upon for the purification of sewage were influenced to a large extent by temperature, and the effect of high temperature was usually favourable. For instance, many chemical changes occurred more rapidly at higher temperatures, and the activities of micro-organisms were also speeded up. The viscosity of water was reduced by a rise in temperature, and sedimentation and filtration performances were thereby improved. On the other hand, high temperatures would militate against the construction of large regional sewerage schemes owing to the rapid development of septic conditions in the sewers. Moreover, undesirable as well as desirable organisms would be encouraged by high temperatures.

Perhaps the Author could indicate how far operating experience bore out those suggestions, and whether sewage treatment works in warm climates could, in general, be designed for slightly smaller capacities than were required in cooler areas.

The mention of the high carbohydrate diet of the African prompted the question as to whether it led to any difficulty in operation of treatment plants apart from the additional sludge-handling and digestion capacity required. In particular, was the activated sludge process suitable for the treatment of that sewage, in view of the tendency of sphaerotilus to thrive on carbohydrates and to cause the bulking of sludge ?

Finally, had the Author had experience with the operation of small septic tanks to treat the sewage from African dwellings ? Could he give any guidance on the capacities required for satisfactory performance ?

The Author, in reply to Mr Bruce, stated that he was convinced that team work was essential in design and operation of sewage disposal work. He had been particularly fortunate in having the Institute of Medical Research, the Medical Officers of Health of Johannesburg, a biologist of the calibre of Dr H. Wilson, and a chemical engineer—Dr J. A. Maclachlan —as colleagues. In Mauritius he also had had a similar team of doctors, biologists, and chemists, whose knowledge had been of inestimable value.

The need for well qualified public health engineers should not be confined to the colonies; the Author sincerely believed that there should be a great call for their services in Great Britain.

The effect of temperature on design data would almost need a complete paper to discuss it fully. To take the case of the sedimentation tanks: the period of retention was much shorter in warm climates—an average of 2 hours—because septic action had to be avoided. The result was that colloidal matter had not the time to settle and had to be dealt with in the percolating filters. Owing to the temperature of the sewage the filters could not only handle the extra colloidal matter but more sewage as well.

Variations of temperature were most important. Cape Town, for instance, with its winter rain and high summer temperature, presented a different problem from Johannesburg with its summer rainfall, lower summer temperature, and dry climate in winter. Mauritius, although only about 700 square miles in area, had a very humid climate, but its ground temperature for 10 feet varied only between 76° and 84° F. throughout the year. That made the sludge digestion process easier because there was no need either to insulate the digestion tanks or heat the sludge.

Regional schemes were not usually warranted (but not because of temperature conditions), except on the Witwatersrand, where nearly half the European population lived within a radius of 36-40 miles from Johannesburg, and the coastal region where the towns were too far apart.

Johannesburg was busy with a regional scheme, Germiston took the sewage of four local authorities, and Cape Town had a large regional scheme. Economics were more important than temperature in South Africa.

Mauritius was designing a regional scheme for six towns, because it was, economically, a better scheme.

Sewage disposal works could, under warm conditions, be smaller than under cooler conditions. Sedimentation tanks and humus tanks were much smaller and the filter bed could, with safety, be 25 per cent smaller.

With regard to activated sludge and carbohydrate content, that process was not so efficient as under British conditions. There were only two large plants in South Africa and both were in Johannesburg. Speaking generally, it was necessary to submit the sewage to 16 hours' treatment. However, by reactivation of the sludge before re-circulation, that could be reduced to 12 hours' treatment. That loss of efficiency was not due entirely to the tendency of sphaerotilus to thrive on carbohydrates. At the altitude of Johannesburg (6,000 feet) the oxygen content of the air was only 80 per cent of that obtaining at the coast.

The Author had no experience of septic tanks for African (Bantu) dwellings under rural conditions because none existed. The closets were either buckets which were emptied three times a week, or pit privies. For African townships in the towns they were either connected to the sewers or supplied with buckets.

Septic tanks were not generally used under urban conditions. When water-borne sewage was allowed, where there were no main sewers, it was usually discharged into a concrete tank and removed by means of a vacuum tank.

Septic tanks might be allowed where the residential site exceeded 2 acres. The size depended upon (a) the water table ; or (b) whether it was in a winter-rainfall area or a summer-rainfall area. The size ranged from 6 cubic feet to 8 cubic feet per person.

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